SCIENCE

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*EDWARD DRINKER COPE, NATURALIST—A CHAPTER IN THE HISTORY OF SCIENCE.**

I.

Bitter constraint, and sad occasion dear
Compels me to disturb your season due;
For Lycidas is dead, dead ere his time
Our Lycidas, and hath not left his peer.

ON the morning of the 13th of April, in a car on my way from a funeral in New York to Washington, a newspaper notice of the death, the day before, of my old friend, E. D. Cope, caught my eye. Shocked by the intelligence, I dropped the paper, and memory recalled various incidents of our long acquaintance.

The threnody of Milton* in commemoration of his friend Edward King also rose to recollection, and the lines just quoted seemed to me to be peculiarly fitted for the great man just dead. He was, indeed, no longer young and had attained his prime,† but he had planned work for many years to come and had well advanced in the execution of some of it. He had truly died before his time and had left no peer; the greatest of the long line of American naturalists was prematurely snatched from science and from friends.

My acquaintance with Cope began in 1859. While looking through the part of the Proceedings of the Academy of Natural Sciences of Philadelphia for the month of April, in which my first paper published by the Academy had appeared, I found one by E. D. Cope 'On the primary divisions of the Salamandridæ.' It seems that the papers by Cope and myself had been passed on by the Committee on Publications on the very same day (April 26th) and appeared in print in juxtaposition. I had not previously

* Address by the retiring President of the American Association for the Advancement of Science at the Detroit Meeting, August 9th.

* Milton, Poems XVII.

† In the extract from Milton's poem, *time* has been substituted for *prime* and *our* for *young*.

heard of the new devotee of science and read his article with as much interest as my own. A well-equipped man had evidently come upon the field and this was the first of the numerous articles that were destined to appear in an uninterrupted flow for nearly four decades. A few months afterwards I met the author in Philadelphia at the Academy. A young man, nineteen years old, about 5 feet 9 or 10 inches high, with head carried somewhat backwards and of rather robust frame, stood before me; he had an alert, energetic manner, a pronounced, positive voice, and appeared to be well able to take his part in any trouble. His knowledge was by no means confined to herpetology, but covered a wide range of science, and his preliminary education had been good. We afterwards met from time to time in Philadelphia and Washington and found we had many sympathies in common and some differences.

In one of our first interviews we had quite an argument on the nature of the family group in zoology, resulting from criticisms I made on the extended scope he had given to that category in the classification of the Salamanders. Another controversy, I remember, had reference to the vertebral theory of the skull. In an article on the venomous serpents, published in the Proceedings of the Academy for 1859, he had defined the group in terms involving the adoption of that theory, and I ventured to dissent from its reality. I had myself been much impressed with it in former days and when 16 years old had copied in colors an illustration of Owen's so-called archetype reproduced in Carpenter's Physiology. Subsequently, however, the fact that there was only an approximation to the realization of it in the most specialized of fishes and not at all among the lower or higher vertebrates, with other considerations, turned me from it, and I gave my reasons for dissent to Cope. Ultimately he admitted the force of the argument and also abandoned the theory at one time so popular in England and America.

Our acquaintance, thus begun in 1859, continued uninterruptedly till death divided us. We rarely met, indeed, that we did not express difference of opinion respecting some subject, but the difference was never of a serious nature and generally little more than sufficient to enliven intercourse.

II.

The future naturalist was born in Philadelphia on the 28th of July, 1840, and the name Edward Drinker was given to him. He was the descendant of a prosperous line long established in Pennsylvania. His father, Alfred, was a man of cultivated literary taste and did much to train his son's mind in early youth. He had retired from active business and lived in luxurious ease in Germantown, a suburb of Philadelphia. There he had formed an arboretum containing most of the American trees which would thrive in the climate of that region. Amidst such surroundings the youthful Cope grew up.

An active and intelligent interest in Nature became manifest at a very early age. When only about seven years old, during a sea voyage to Boston with his father, the boy is said to have kept a journal which he filled with drawings of 'jelly fish, grampuses and other natural objects seen by the way.' When eight and a half years old he made his first visit to the Museum of the Academy of Natural Sciences of his native city; this visit was on the "21st day of the 10th Month, 1848," as entered in his journal. He brought away careful drawings, measurements and descriptions of several larger birds, as well as of the skeleton of an Ichthyosaurus. His drawing of the fossil reptile bears the explanatory legend in Quaker style: "two of the sclerotic plates look at the eye—thee will see these in it."

At the age of ten he was taken upon a voyage to the West Indies.* What were the impressions he derived from that voyage we have not been told. But what has been communicated amply justified Professor Osborn in his declaration that "the principal impression he gave in boyhood was of incessant activity in mind and body, reaching in every direction for knowledge, and of great independence in character and action." His school education was mostly carried on in the Westtown Academy, a Quaker institution about 23 miles west of Philadelphia. One of his instructers was Dr. Joseph Thomas, a well known literary worker of Philadelphia and future author of a 'Universal Pronouncing Dictionary of Biography and Mythology' (1870), and said to be an 'excellent linguist.' Under his guidance Cope obtained a passing knowledge of Latin and Greek. He appeared to have had no instruction in any biological science and had no regular collegiate training. He did, however, enjoy the advantage of "a year's study (1858–9) of anatomy and clinical instruction at the University of Pennsylvania," in which the illustrious Leidy was professor of anatomy. But, in the words of his literary executor (Professor H. F. Osborn), "it is evident that he owed far more to paternal guidance in the direct study of nature and to his own impulses as a young investigator than to the five or six years of formal education which he received at school. He was especially fond of map drawing and of geographical studies."

While a school boy he relieved his studies of the classics and the regular course in which boys of his age were drilled by excursions into the fields and woods. Reptile life especially interested him, and he sought salamanders, snakes and tortoises under rocks, stones, fallen trees and layers of leaves, as well as in the ponds and streams of his vicinage. The trophies of his excursions were identified from descriptions in the works in which they were treated, as well as by comparison with identified specimens in the museum of the Academy. He early and almost without guidance learned to use the library and collection of the Academy, although he did not become a member until he came of age in 1861.

Cope's first contribution to the Proceedings of the Academy appeared in the part covering April and was 'On the Primary Divisions of the Salamandridæ, with descriptions of the New Species.'* In this maiden paper he instituted important modifications of the systems previously adopted in the United States. He soon afterwards catalogued the serpents preserved in the museum of the Academy of Natural Sciences and likewise improved upon the systems previously in vogue. He continued with various papers, describing new species and giving synopses or brief monographs of sundry genera of lizards and anurous amphibians.

For five years his publication was confined almost exclusively to the reptiles and amphibians. (The continuity was only interrupted once in 1862, when he described a new shrew caught by himself in New Hampshire.) Not until 1864 did he begin to extend his field. In that year he described various fishes and a supposed new whale, and gave his first contribution to paleontology in the description of the stegosaurian amphibian called *Amphibamus grandiceps*. But although his attention had become thus divided, he never lost his interest in herpetology and continued to the end of his life to devote much attention to that department. His studies extended to every branch of the subject, covering not only specific details and general taxonomy, but also the consideration of anatomical details, the modifications of different organs, geographical distribution,

* Osborn in SCIENCE, N. S., V., 706.

* Proc. Acad. Nat. Sci. Phila., 1859, pp. 122-128.

chronological sequence, genetic relations and physiological consequences. So numerous were his memoirs, so entirely did he cover the field of herpetology, and so marked an impression did he make on the science, that he was well entitled to apply to himself the boast of the Vergilian hero, '*Pars magna fui.*'

In his earliest essays he manifested the independence and critical spirit which were so characteristic of him later. One knowing all the circumstances of the case may be amused in coming across a passage expressed in the tones of a veteran published by him when 20 years old: "In proposing the name Zaocys * * * we are giving expression to an opinion *long held by us* as to the unnatural association of species in the so-called genus *Coryphodon* * * *. In it we find cylindrical terrestrial species united with compressed subarboricole species, upon a peculiarity whose value as an index of nature appears to us entirely imaginary. The very nature of the *coryphodontian* type of dentition, as distinguished from the *isodontian* and *syncranterian*, would lead us to infer its inconstancy;" and so on.* Bold as was the criticism of such herpetologists as Duméril, Bibron and Günther, it was justified by the facts, and the young author's conclusions have received the endorsement of the best succeeding herpetologists, including even the latest author criticised.

In 1863 he paid a visit to Europe, partly for the benefit of his health which had suffered from overwork, and partly for the purpose of seeing the great museums of England, France, Holland, Austria and Prussia. Notwithstanding his ailments, he made good use of his time abroad and systematically examined the collections of reptiles in the chief centers of science. He did not even restrict his studies to herpetology, but extended them to various other subjects.

*Proc. Acad. Nat. Sci., Phila., 1860, p. 563.

On his return from Europe, in 1864, he was appointed professor of natural science in Haverford College, an institution chiefly supported by Quakers, but retained the position only three years. During this time, in 1865, he married Miss Annie, daughter of Mr. Andrew Pim, of Chester county, Pa.

In and after 1864, too, he enlarged the range of his studies and publications and also extended them to ichthyology, mammalogy and paleontology. He had always been interested in the philosophical aspects of science and early adopted the conception of descent with modifications to account for the variations of animals and the differentiation into species and higher groups, and in 1869 began to give expression to his peculiar views.

On the death of his father he became heir to a considerable fortune. Part of this was invested in mines which for a short time gave promise of good returns, but, it is said, the majority of the stock was held by others, and owing to the incapacity of superintendents and the operations of the controlling stockholders he lost his interests. While in the enjoyment of his fortune he spent large amounts in collections and personally conducted or sent out expeditions to various places. One of the most important was sent to South America. He filled a large house from cellar to topmost story with his collections and resided in an adjoining one.

In 1871 he conducted an expedition to Kansas and especially investigated the Cretaceous beds of that State and collected their fossils. In 1872 and 1873 he became connected with the U. S. Geological Survey and for the fossils visited Wyoming in the former year and Colorado in the latter. In 1874 he joined the survey under the command of Lieut. Wheeler, of the Engineers, and explored New Mexico.

The collections made during these expeditions were large and the unwearied in-

dustry and energy, as well as cares, of Cope were rewarded with many well-preserved fossils. These were described in many communications to the Academy of Natural Sciences and the American Philosophical Society, and later in large volumes published by the general government as reports of the respective surveys with which he was connected.

The various investigations thus opened were continued through the succeeding years. His collections continued to grow in spite of reduced means. He refused even to sell portions for which he was offered liberal sums and, at the cost of personal discomfort, held on to them and made his home, for much of the time, in the midst of them, having sold his residential house but kept his museum.

In 1878 he purchased the rights of the proprietors of the *American Naturalist* and removed it to Philadelphia. Professor Packard, one of the original proprietors, cooperated with him in the editing of it for some years, and he was also assisted by various eminent specialists. In this journal numerous articles of all kinds, including reviews and editorial comments, were published by him. His last words appeared in numbers issued after his death, the leading article in the number for June having been written shortly before his death; it treats of the remarkable mammals of South America, known as Toxodontia.

In 1886 he received an appointment to a chair in the University of Pennsylvania and became professor of geology and paleontology. Such a man naturally awakened the interest of apt pupils, and he was a facile and entertaining lecturer. From the stores of a rich memory he could improvise a discourse on almost any topic within the range of his varied studies. His views were so much in advance of those in any text-book that for his own convenience, no less than for the benefit of his pupils, he felt compelled to prepare a 'Syllabus of lectures on geology and paleontology,' but only 'Part III., Paleontology of the Vertebrata,' was published. It appeared in 1891 and is still a valuable epitome of the classification of the vertebrates, recent as well as fossil, giving in dichotomous tables the essential characters of all the groups above families and also the names of all the families. His own industry and investigations did much to render this antiquated in even six years and a new edition or work became necessary. "Upon the Tuesday preceding his death he sent to the press an elaborate outline of his University lectures containing his latest ideas of the classification of the Vertebrata."*

The enormous mass of publications constantly flowing from his own pen might lead one unacquainted with the author to suppose that he was probably a recluse, but there were few men of his intellectuality who were less disposed to seclude themselves. He enjoyed and gave enjoyment to intellectual company and was a brilliant conversationalist. He was especially fond of academical meetings and was an unusually frequent attendant at the meetings of the American Association as well as of the National Academy of Sciences. His election to the Presidency of the American Association was highly esteemed by him and doubtless his address would have been a notable one.

In February (1897) Cope's health became seriously affected by nephritic disorder, which, it is said, 'might possibly have been remedied by a surgical operation,' but this he would not submit to. Notwithstanding failing health, he continued active almost to the last. Finally the insidious disease invaded his entire system and he died on the 12th of April, in the room he had long used as a study, surrounded by the objects of his life-long attentions.

* Osborn in SCIENCE, May 7, p. 705.

Such were the chief episodes of Cope's individual life; the facts known are few and the record belongs rather to his family than to us. But Cope's real life was in his work and to the consideration of that work we may now proceed. Let us adopt the order in which he took up the subjects of his investigations and successively look into his contributions to herpetology (III), ichthyology (IV), mammalogy (V) and paleontology (VI); we may then examine his philosophical views and especially those relating to evolution (VII); finally we may attempt to forecast the position he is destined to enjoy in the history of science (VIII). To know him as he was we must recognize his weakness as well as his strength. He himself has wished this and has asked in the spirit of the Moor:

> Speak of me as I am; nothing extenuate,
> Nor set down aught in malice.

III.

The extent of Cope's contributions to herpetology have been referred to. Herpetology was his first love and continued to be the favorite branch of science to his life's end. His impress on it was, in some respects at least, greater than on any other of the sciences he cultivated, and doubtless the systems he introduced, with some modifications, will be the most lasting. He found herpetology an art; he left it a science; he found it a device mainly for the naming of specimens; he left it the expression of the coordination of all structural features. The reformations he effected in the classification of the anurous amphibians and the saurian reptiles were especially notable.

The anurans had been chiefly differentiated in groups on account of the most superficial characters. Such were the modes of fixation of the tongue or its absence, the development of disk-like expansions of the tips of the toes, or simply attenuated toes, and the presence or absence of teeth in a jaw. Cope proceeded to investigate the group in an anatomical manner and reached entirely new conclusions. He found that important differences existed in the structure of the sternum, and especially in the connection of the lateral halves. In the common toads and tree toads of Europe and North America the so-called clavicle and coracoid of each side are 'connected by a longitudinal arched cartilage which overlaps that of the opposite side,' while in the common frogs the clavicles and coracoids of both sides are connected by a single median cartilage. The former type is now known as the arciferous and the latter as the firmisternal. Although Cope was the first to appreciate the significance of those characters, he did not at once fully realize their morphological value, the name Arcifera having been originally applied by him only to types of that group having teeth. Ultimately he did so, and his views have stood the test of time and the latest critical investigations. He also found that the characters so revealed served to fix the places in the system of the groups in question. In their early stages the Firmisternials (or frogs and their relations) have the shoulder-girdle movable, and thus resemble the Arcifers (toads, etc.), which have the opposite halves movable during their whole lifetime; thus it became evident that the latter are the lowest or most generalized forms, and the former more advanced and higher in the system. The development of teeth, which had been supposed by the earlier systematists to be of paramount value, and which Cope, following in their footsteps, had also originally unduly valued, has been found to be of quite subordinate importance.

The lizards were also in former times distributed into families and other groups on account of variations in superficial or external characters, such as the form of the tongue, the arrangement of the scales and the development of legs and feet. Cope

dissected examples of all the types he could obtain and found that such superficial characters were often misleading, and he proceeded to arrange them with reference to the preponderance of all characters. The structure of the cranium especially was analyzed, and the variations and concordances in the development of various bones were tabulated. These characters were supplemented by others derived from the vertebræ, the shoulder girdle, the teeth, the tongue and the pholidosis. Familiarity with his subject enabled him almost instinctively to assess the relative values of the different characters, and he obtained fitting equations which resulted in a system which has received the approbation of the most competent judges to the present time.

The extent of Cope's influence on herpetology may be to some extent inferred from the catalogues of the richest collection of reptiles and amphibians in existence —the British Museum's. Descriptive catalogues of both the Anurans and Saurians have been published at different times. In the early catalogues are adopted the views current at the dates of publication—1845 for the lizards; 1858 for the batrachians. New editions were published many years later and the systems of Cope were adopted with slight modifications. In his catalogue of the *Batrachia salientia* Mr. Boulenger, the author, remarked that it appeared "undeniable that the principles of classification laid down by Mr. Cope are more in accordance with the natural affinities of the genera of tailless Batrachians than those employed by other authors; this is amply proved by all we know of their geographical distribution, development and physiology."

In an article* published in advance of his catalogue of the lizards, Boulenger states that the old classifications are, 'on the whole, as unnatural as can be' and that, "like Cope, whose lizard families I regard as the most natural hitherto proposed, I shall lay greater stress on osteological characters and on the structure of the tongue."

It was a long time, however, before Cope's views became popular. Even anatomists of repute refused to follow him. One* of them, for example, admitted that "skeletal characters are, indeed, most valuable ones in leading us to detect the deepest and truest affinities of vertebrate animals, but [he urged] these affinities once found, it is very desirable that zoological classification should not, if it can possibly be avoided, *repose* upon them only, but rather on more external and more readily ascertainable characters." He, therefore, ventured 'to propose a classification derived from that of Dr. Günther.'

Cope replied† by a fierce review of the work of Dr. Günther, and concluded with the utterance that such views 'will only interfere with the progress of knowledge if sincerely held and believed.'

But such views were evidently sincerely believed and they did retard the progress of science. An eminent Russian herpetologist objected to the use of anatomical characters. He especially protested against those employed by Boulenger after Cope to the grouping of the lizards, and Mr. Boulenger considered it incumbent on him to defend the practice of using such characters; ‡ he aptly replied that the use of 'purely external characters * * * does not meet the requirements of modern science,' and that classifications are not made simply 'for the convenience of beginners.'

At last, however, the principles of classification adopted by Cope have become gen-

*Synopsis of the families of existing Lacertilia. Ann. and Mag. Nat. Hist. (5), XIV., 117.

*Mivart in Proc. Zool, Soc., London, 1869, p. 281.

†Cope in Am. Journ. Sci. (3), I., p. 203.

‡Boulenger in Ann. and Mag. Nat. Hist. (5), XIX. 385.

erally accepted, and doubtless this was in no small degree hastened by their application to all the amphibians and reptiles by Boulenger.

Cope's attention to the extinct reptiles was excited by the examination and consideration of a Carboniferous lizard-like amphibian which he was requested in 1865 to report upon. It was a new species which he named *Amphibamus grandiceps* and considered to be the type of a new order to which the name Xenorachia was applied, but which he subsequently referred to the new comprehensive order Stegocephali.

He sought for specimens of the extinct species with as much enthusiasm as he had for the recent. Extinct and living he considered together and light was mutually reflected from the two to guide him in the perfection of the entire system. In 1869 he gave expression to the results of his studies in a well illustrated 'Synopsis of the Extinct Batrachia, Reptilia and Aves of North America.' This was supplemented in 1874 by addenda and a 'Catalogue of the air-breathing Vertebrata from the coal measures of Ohio.'

A rich field was opened to him in 1877, when he received the first instalment of reptilian remains from Texas, which were at first considered to be of Triassic age, but subsequently determined to be Permian. Successive instalments of amphibian as well as reptilian skeletons enriched his collection, and his investigations revealed a new and wonderful fauna rich in species and often differing widely from any previously known. These were described in many articles. The results for the amphibians were summarized in 1884 in a memoir on the 'Batrachia of the Permian period of North America.'

The Permian amphibians were found to vary much in the composition of their backbones. Instead of having single centra arranged in a continuous row as in existing Vertebrates, they had distinct bones on which were devolved portions of the functions fulfilled by the centra of higher Vertebrates. Some had 'the vertebral bodies represented by three segments each, a basal intercentrum and two lateral pleurocentra;' these were named 'Ganocephali' and 'Rhachitomi.' Some "differ remarkably from all other Vertebrata in having between the centra another set of vertebral bodies, so that each arch has two corresponding bodies;" these were called 'Embolomeri.'

In tracing the development of these bones, Cope came to the conclusion that they were only partially represented in higher or more specialized types; they did not become consolidated, but one or the other became reduced and finally lost or at least greatly atrophied. In the living amphibians the vertebral centra are homologous only with the intercentra, while, on the contrary, the centra of the reptiles, birds and mammals are represented by the pleurocentra of the Rhachitomes.

The studies of Cope on those classes which had earliest attracted his attention were more nearly completed than for any others. Many years ago he had contemplated the publication of monographs of the amphibians and reptiles of North America and happily he had at last finished his work.

In 1889 his monograph of the 'Batrachia of North America' was given to the world as a Bulletin of the United States National Museum (No. 34). It forms a goodly volume of 525 pages illustrated by 86* plates and 120 figures inserted in the text. No large country has a more elaborate and scientific exposition of the class than is given in this volume. A synopsis is furnished of all the families and genera wherever found, and detailed descriptions are supplied for all the groups and species

* The last plate is numbered 86, but five were cancelled, 80, 81, 82, 84 and 85.

represented in the zoological realm of North America, 31 genera and 107 species are recognized, and of these Cope had first made known about a quarter, 7 of the genera and 27 of the species having been described by himself. Shortly before his death, and during his last visit to Washington he delivered to the National Museum the report on all the reptiles of North America which he had been long preparing. This was prepared on the model of his 'Batrachia of North America,' but will, of course, be a much larger work, inasmuch as there are nearly three times as many reptiles as Batrachians.* His last elaborate memoirs dealt with special anatomical features of the serpents and lizards, which he examined with the view of perfecting the system of those groups.

IV.

In 1864 he became especially interested in the fresh-water fishes of the United States, and then as well as in succeeding years published enumerations and descriptions of many species. His first papers in 1864 and 1865 were 'On a blind Silurid from Pennsylvania' and a 'Partial catalogue of the cold-blooded Vertebrata of Michigan;' in 1868 he published 'On the distribution of fresh-water fishes in the Allegheny region of southwestern Virginia,' and in 1869 appeared a 'Synopsis of the Cyprinidæ of Pennsylvania.' In addition to these, various minor papers were published and in some of them marine forms were considered.

When in Europe Cope had purchased a large collection of skeletons of fishes from all parts of the world prepared by Professor Joseph Hyrtl, of Vienna, one of the most skillful practical anatomists of the day. He had a number of other skeletons made to represent missing types. With these as a basis he proceeded to recast the classification of fishes. The first contribution to the subject was embodied in an introductory chapter of his 'Contribution to the Ichthyology of the Lesser Antilles,' published early in 1871.

The same chapter, with the same title, 'Observations on the Systematic Relations of Fishes,' but with some modifications and additions, was later published in the Proceedings of the American Association for the Advancement of Science for 1871. This was a notable paper and replete with original observations of value. It was not, however, up to the standard of his work on amphibians and reptiles. The subject, indeed, was too vast and only a superficial examination was made of special parts. It was not a classification based on the examination of the entire structure, but rather an exposition of the development of a few particular characters, which more experience subsequently convinced him were of less value than he had supposed. Nevertheless, in some respects the proposed classification was much in advance of those previously adopted, and useful hints were given for the further improvement of the system.

Later Cope followed up this attempt at the reformation of the ichthyological system with several others especially treating of extinct types. One of them, 'On the classification of the extinct fishes of the lower types,' was published in the Proceedings of the American Association for 1877. The results of his studies were summarized, in 1889, in 'A synopsis of the families of Vertebrata,' and two years afterwards (1891) with modifications, in an article 'On the non-actinopterygian Teleostomi.' These results were very valuable and attention was for the first time directed to the importance and morphological significance of the skeletal fin structures of the ancient fishes long confounded under the name of Gan-

* Cope's monograph of the reptiles will not include the tortoises, those having been left to Dr. G. Baur to monograph.

oids. Instead of this single order (or subclass) of the old systematists, he named four superorders of the Teleostomi or true fishes,' and recognized seven orders, including the old ganoids after eliminating the Lepidosteids and Amiids, which were referred to the Actinopterygians. Only two of the seven orders are represented by existing forms—one (*Cladistia*) by the bichirs of Africa and the other (*Chondrostei*) by the sturgeons.

His work on the extinct fishes was incomparably better than any that had been done before in the United States. He far surpassed all his predecessors, not only by his knowledge of morphological details manifest in the extinct as well as living forms, but by his keen philosophical instinct and taxonomic tact. But this philosophical instinct was sometimes at fault, and occasionally he indulged in the wildest speculations, for which he has, not unjustly, been taken to task. But even his blunders were the result of the facility of his mind in seizing and adapting the latest utterances of science. One notorious case may be given. The great Russian embryologist Kowalevsky published a memoir sustaining the thesis that the Tunicates were members of the vertebrate phylum and that the larval stage of most of the species had the homological equivalent of the backbone of the true vertebrates. Cope foresaw the morphological consequences of this view and sought the vertebrates nearest the Tunicates. He settled upon some strange forms of the Silurian and Devonian times known as Pteraspids and Cephalaspids. They were the earliest known of vertebrates and therefore likely to be the most primitive in structure. Most of them had a shell-like encasement, composed of bone-like plates. He happened to find illustrations of the living *Chelyosoma*, a true Tunicate having a system of plate-like indurations of the integument, somewhat similar in appearance to those of some of the ancient fishes. It was assumed that this mere superficial similarity indicated genetic relationship. To those acquainted with the structure of *Chelyosoma* this approximation seemed strange indeed; its anatomy was known and the form is simply a well marked relation of the typical ascidiids, but highly specialized by the development of integumentary plate-like horny indurations. Histologically and otherwise they were very different from the plates of the extinct armored vertebrates. Cope's guess was simply the result of the tendency to jump at conclusions which he was constantly obliged to curb, and unfortunately he rushed into print before he had time to think. He soon reconsidered the case with calmer mind, and abandoned his hypothesis. Few men were ever more willing to reconsider evidence and retrace false steps than he.

In spite of errors of detail and somewhat hasty generalization the ichthyological labors of Cope were unusually valuable contributions to science, and the progress of ichthyology has been much accelerated, not only by these labors, but by the investigations they challenged.

V.

Cope's attention was early drawn to the mammals. His first published article (1863) was a description of a supposed new Shrew found in New Hampshire, and in 1865 he described various cetaceans. In 1868 he began the collection and investigation of the fossil mammals of the western territory, and thenceforward devoted the larger share of his attention to the description and restoration of the numerous new species which he from time to time brought to light. The previous investigators of the extinct mammals of America had almost exclusively confined themselves to descriptions and illustrations of the crania and dentition, but a new era was intro-

duced when Marsh and Cope sent out exploring expeditions or themselves collected. No parts of skeleton were neglected; all were collected. Gradually the numerous bones from different parts of the skeleton were identified, and finally many of the beasts of old were resurrected into skeletons almost as complete as those just divested of muscles.

The discoveries resulting from such thorough work quite modified or even overturned old conceptions. It became evident that there was a great contrast between the development of the mammals and that of invertebrates, and even, though in a less degree, of fishes. It appeared that there was a much more rapid process of evolution for the mammals than for the lower classes. All the mammals of the oldest of the Tertiary periods were strange and very unlike those of recent times, and no descendants of even the same families lived to be the contemporaries of civilized man. The views of the founder of vertebrate paleontology were also to a considerable extent subverted. Cuvier taught that there was always a coordination between the various systems of the animal frame and that from the remains or impress of one part the approximate structure of the other parts could be inferred. He even pushed this doctrine to such an extreme that he overlooked some obvious counter-facts. One such case is so remarkable because it orginated with Cuvier and was endorsed by Huxley* that it is worthy of mention here, and Huxley's introduction to it and translation of it may be given. Huxley himself protests against the too literal application of Cuvier's law and recalls Cuvier's own reserve:

"Cuvier, the more servile of whose imitators are fond of citing his mistaken doctrines as to the nature of the methods of paleontology against the conclusions of logic and of common sense, has put this so strongly that I cannot refrain from quoting his words.*

"But I doubt if any one would have divined, if untaught by observation, that all ruminants have the foot cleft, and that they alone have it. I doubt if any one would have divined that there are frontal horns only in this class; that those among them which have sharp canines for the most part lack horns.

"However, since these relations are constant, they must have some sufficient cause; but since we are ignorant of it, we must make good the defect of the theory by means of observation. It enables us to establish empirical laws, which become almost as certain as rational laws, when they rest on sufficiently repeated observations; so that now, whoso sees merely the print of a cleft foot may conclude that the animal which left this impression ruminated, and this conclusion is as certain as any other in physics or morals. This footprint alone, then, yields to him who observes it, the form of the teeth, the form of the jaws, the form of the vertebræ, the form of all the bones of the legs, of the thighs, of the shoulders, and of the pelvis of the animal which has passed by. It is a surer mark than all those of Zadig."

The first perusal of these remarks would occasion surprise to some and immediately induce a second, more careful reading to ascertain whether they had not been misunderstood. Some men, with much less knowledge than either Cuvier or Huxley, may at once recall living exceptions to the positive statements as to the coordination of the 'foot cleft' with the other characters specified. One of the most common of domesticated animals—the hog—would come up before the 'mind's eye,' if not the actual eye at the moment, to refute any such correlation as was claimed. Nevertheless, notwithstanding the fierce controversial literature centered on Huxley, no allusion appears to have been made to the lapsus. Yet every one will admit that the hog has the 'foot cleft' as much as any ruminant, but the 'form of the teeth' and the form of some vertebræ are quite different from those of the ruminants, and of course the multiple stomach and adaptation

*Huxley, 'Introduction to the Classification of Animals,' 1869, in first chapter 'On Classification in General.'

*Ossemens fossiles, ed, 4me, tome, 1r, p. 184.

for rumination do not exist in the hog. That any one mammalogist should make such a slip is not very surprising, but that a second equally learned should follow in his steps is a singular psychological curiosity.

I need scarcely add that the law of correlation applied by Cuvier to the structures of ruminants entirely fails in the case of many extinct mammals discovered since Cuvier's days. Zadig would have been completely nonplussed if he could have seen the imprint of an Agriochœrid, a Unitatherid or a Menodontid.

I have given this quotation for two reasons: first, to indicate how the increase of our knowledge has revolutionized old conceptions; and second, to show how even the ablest of men may stumble.

Cope has been much criticised for the mistakes and false generalizations he made. Unquestionably he did make many. But error seems to be inseparable from investigation, and if he made more than the other great masters he covered more ground and did more work. He was also, it must be admitted, more hasty than some others in that he availed himself of the more frequent means of publication he enjoyed.

The great merit of Cope's work on mammals is that he always considered the old and new—the extinct and recent—forms together. He refused to be bound by consistency or by precedent, either set by himself or others. Fresh discoveries opened new vistas to him, and he modified his views from time to time and as often as he received new evidence.

He introduced many new families in the system and sought to improve the system by the comparison of all the elements of the skeleton. He came to the conclusion that the affinities of the ungulate quadrupeds was best expressed by the manner of articulation of the bones of the carpus and tarsus; he associated those having the 'carpal and usually tarsal bones in linear series' in a great order which he called Taxeopoda, and contrasted them with the Proboscidea and typical Ungulata, which he named anew Diplarthra. In the Taxeopoda he gathered many extinct families and associated with them forms of the existing fauna known as the Hyracoidea, Daubentonioidea, Quadrumana and Anthropomorpha. I cannot altogether assent to this collocation inasmuch as I think the common characteristics of the three groups last mentioned—especially the structure of the brain and the development of the posterior cornua of the ventricles as well as calcarine sulci—justify the old order Primates. Nevertheless an important character was first appreciated in the composition of the podial bones, and fresh insight was obtained into the relations of ancient types.

I can only name a few more of Cope's discoveries in this connection. One was the generalization of 'tritubercuły,' or the original development of three tubercles to molar teeth, and that subsequent modifications of the corresponding teeth were based on this original plan. Another was the remarkable *Phenacodus* of the Eocene, which was considered to be nearly in the line of descent for the ungulates as well as the series culminating in man and which led him to the conception of the taxeopodous group.

The past history and genealogy of the Camels and their relations were likewise elucidated. In the present epoch only two nearly related types exist separated by half the globe—the true camels of central and northern Asia and the llamas of the Peruvian Andes. Cope revealed numerous species from various Tertiary beds and showed that the type was originally richly developed in America.

VI.

Paleontology from more than one point of view may be divided into Invertebrate and

Vertebrate. The subjects of the former are generally to be found in an approximately complete condition so far as the exterior is concerned, and early attracted the attention of investigators, often little familiar with recent zoology, and received names. The subjects of the latter—especially the higher types, as mammals, birds and reptiles—are rarely found, except in a fragmentary condition. Special knowledge of osteology, even to its minutest details, is requisite to successfully deal with such remains. Consequently the fossil vertebrates of the United States were neglected and left to the few who had cultivated the requisite knowledge to deal with them.

Another reason existed for the tardy attention to Vertebrate paleontology, which continued till nearly the last quarter of our present century in the United States. No deposits containing many fossil vertebrate remains had become known in the East. Zoologists interested in the past and in the genealogy of existing forms lamented the poverty of the United States, which contrasted with the richness of some parts of Europe. It was even thought that there was no hope of finding here such trophies of the past as the beds of the Paris Basin or those of Grecian Pikermi had yielded to European paleontologists. But all this was to be changed. Rumor had long before hinted that numerous skeletal remains could be found in certain parts of the wild West, but the information was very vague. Enough was known, however, to induce Professor Marsh to visit certain deposits he had heard of. In 1870 he explored an Eocene lake-basin in Wyoming, drained by the Green river, the main tributary of the Colorado, and therein found numerous bones belonging to almost all parts of the skeleton, of some remarkable gigantic mammals which he called Dinocerata. The results of this exploration interested Cope in the highest degree. He visited the same region in 1872, and thenceforth his attention to the Vertebrate paleontology of the Western States and Territories was never interrupted. An intense rivalry arose between Professor Marsh and himself which in time, it must be confessed, became very bitter. Nevertheless, as in most quarrels respecting facts, investigations were provoked by mutual recriminations which resulted in a more speedy accumulation of data and a more critical examination of those data than would have been likely under less perturbed conditions. Most of those data relate to morphological and anatomical considerations, and therefore belong rather to mammalogy and herpetology than to geology.

The relations of the ancient forms to each other in point of time; to those of other lands, and to those whose remains were embedded in other rocks, had necessarily to be investigated. The earliest conclusions of Cope were brought together and published in 1879 in a memoir on 'The Relations of the Horizons of Extinct Vertebrata of Europe and North America.'* He attempted therein to synchronize, or, rather, homotaxially correlate the various ancient fauna of North America and 'West Europe' from the 'Primordial' to the 'Pliocene.' Naturally the greater part of the memoir was devoted to the consideration of the Tertiary divisions; of these he admitted for the American form six primary divisions, and four of these were dichotomously subdivided for the time. Of the primary divisions three were referred to the Eocene, one (White River) to the Oligocene, one (Loup Fork) to the Miocene, and one to the Pliocene. The exposition thus made represents views not very different from those now held, although, of course, modifications in details have since been necessary.

The evolution of the various animal, and especially mammalian types, was also con-

* Bull. U. S. Survey Terr., V., 33-54.

tinually the subject of Cope's researches, and he attempted to trace the passage from those of the most ancient periods to those of later ones.

VII.

Cope was not satisfied with the study of morphological details or simple taxonomy. He aspired to know how animals came into existence; why they varied as they did, and what laws determined their being. His was an eminently philosophical mind, but at the same time with a decided tendency to metaphysical speculation. In one of his earliest papers he manifested this tendency and it persisted through life. It is with much hesitation that I venture to give an exposition of his most salient views, for I must confess I do not altogether like his philosophy and am able to subscribe to it only in part. I cannot but wish that one of his numerous disciples could have been chosen for this task. But I cannot pass it by, for it is the most characteristic feature of Cope's work and the one he most esteemed.

Cope began his public scientific career, it will be remembered, in the same year in which Darwin's long studies had fructified into his 'Origin of Species.'

As was quite natural with his keen instincts, Cope early adopted the doctrine of transmutation of species and recognized the truth that all the animals of the present epoch are descendants from those of past times with modifications which separate them as species, and eventually as representatives of genera, of families and orders differing from the earlier ones as we retrace the steps of Time farther and farther back. He was not, however, satisfied with Darwin's theory, and denied that natural selection was a sufficient factor for differentiation. He would not admit that animals were passive subjects and that the slight variations which were manifest in the progeny of species were sufficient to enable nature to select from and to fit for future conditions. He contended that the volition and endeavors of an animal had much to do with future progeny as well as its own brief life. In short, he claimed that characters acquired by animals through their own efforts or forced on them by various external agencies or accidents might be transmitted to their offspring. He further, first in a in a chapter in his 'Synopsis of the Cyprinidæ of Pennsylvania' outlined, and later, in 'The Origin of Genera,' he elaborated, a peculiar theory characterized mainly by what he called (with Professor Hyatt) 'the law of *acceleration* and *retardation*' in development. Darwin complained that he could never understand this law, and Cope complained that Darwin had not stated his views correctly in an attempted abstract. I therefore give Cope's views, restated in his own language, summarizing them years afterwards. "The following doctrines," he he says, "were taught:"

"First, that the development of new characters has been accomplished by an *acceleration* or *retardation* in the growth of the parts changed. This was demonstrated by reference to a class of facts, some of which were new, which gave ground for the establishment of the new doctrine.

Second, that of *exact parallelism* between the adult of one individual or set of individuals and a transitional stage of one or more other individuals. This doctrine is distinct from that of inexact parallelism which had already been stated by von Baer. And that this law expresses the origin of genera and higher groups, because,

Third, they can only be distinguished *by single characters* when all their representatives come to be known.

Fourth, that genera and various other groups have descended, not from a single generalized genus, etc., of the same group, but from corresponding genera of one or more other groups. This was called the doctrine of *homologous groups*.

Fifth, the doctrine that these homologous groups belong to different geological periods, and,

Sixth, to different geographical areas, which, therefore, in some instances, are,

Seventh, related to each other in a successional way like the epochs of geological time.

"Of these doctrines it may be observed that the first and second are now the common property of evolutionists, and are recognized everywhere as matter of fact. The names which I selected to express them have, however, only come into partial use. The author believes that, although the doctrine was vaguely shadowed out in the minds of students prior to the publication of this essay, it had not previously been clearly expressed, nor been reduced to a demonstration. Of the truth of the doctrine the author is more than ever convinced, and he believes that paleontological discovery has demonstrated it in many instances, and that other demonstrations will follow. The fourth proposition (that of homologous groups) is now held as a hypothesis explaining the phylogeny of various groups of animals. For the descent of one homologous group from another, the term *polyphyletic* has been coined. It remains to be seen whether the doctrine is of universal application or not. That homologous groups belong to different geological horizons, as stated under the fifth head, has been frequently demonstrated since the publication of the essay. That the sixth proposition is true in a certain number of cases is well known, and it follows that the seventh proposition is also true in those cases. The latter hypothesis, which was originally advanced by Professor Agassiz, is, however, only partially true, and the advance of paleontological study has not demonstrated that it has had a very wide application in geological time.

"A proposition which was made prominent in this essay was that the prevalence of non-adaptive characters in animals proves the inadequacy of hypotheses which ascribe the survival of types to their superior adaptation to their environment. Numerous facts of this kind undoubtedly indicate little or no activity of a selective agency in nature, and do point to the existence of an especial developmental force acting by a direct influence on growth. The action of this force is the acceleration and retardation appealed to in this paper. The force itself was not distinguished until the publication of the essay entitled 'The Method of Creation' [1871], where it was named growth-force, or bathmism. The energetic action of this force accounts for the origin of characters, whether adaptive or non-adaptive, the former differing from the latter in an intelligent direction, which adapts them to the environment. The numerous adaptive characters of animals had by that time engaged the attention of the author, and he found that they are even more numerous than the non-adaptive. Some of the latter were accounted for on the theory of the 'complementary location of growth-force.'"

We can only consider the 'law of acceleration and retardation.' Again it behooves us to seek his own definition:

"*a.* The succession of construction of parts of a complex was originally a succession of identical repetitions; and grade influence merely determined the number and location of such repetitions.

"*b. Acceleration* signifies addition to the number and location of such repetitions during the period preceding maturity, as compared with the preceding generation, and *retardation* signifies a reduction of the number of such repetitions during the same time.*

His meaning may best be inferred from his application to mankind. This was done in the following terms in 1872:†

"Let an application be made to the origin of the human species It is scarcely necessary to point out at the start the fact, universally admitted by anatomists, that man and monkeys belong to the same order of Mammalia, and differ in those minor characters, generally used to define a 'family' in zoology.

"Now, these differences are as follows: In man we have the large head with prominent forehead and short jaws; short canine teeth without interruption behind (above); short arms, and thumb of hand not opposable. In monkeys we have the reverse of all these characters. But what do we see in young monkeys? A head and brain as large relatively as in many men, with jaws not more prominent than in some races; the arms not longer than in the long-armed races of men, that is, a little beyond half way along the femur. * * * At this age of the individual the distinctive characters are therefore those of *homo*, with the exception of the opposable thumb of the hind foot, and the longer canine tooth. * * *

"Now in the light of various cases observed, where members of the same species or brood are found at adult age to differ in the number of immature characters they possess, we may conclude that man originated in the following way: that is, by a delay or retardation of growth of the body and fore limbs as compared with the head; retardation of the jaws as compared with the brain case, and retardation in the protrusion of the canine teeth."

There is good reason for thinking that fallacy is involved in this argument and that quite a different interpretation should be put on the evolution of the characters in

*Proc. Am. Phil. Soc., 1871; Origin of Fittest, p. 182.

†Penn. Monthly Mag., 1872; Origin of the Fittest, p. 11, 1887.

question. It is not the fore limbs that are retarded in man, but the hind limbs have become enlarged (compare the adult and the infant). There is not retardation of the jaws, but a special teleological adaptation. Man has for the most part at least discontinued the use of his teeth for warfare, and as a result of diminished use the canines have become reduced and the diastemata of the dental series been obliterated. The brain has grown after birth and become enlarged, and as a consequence the brain case has extended forward—the reverse of what occurs in the apes. Concomitantly with the diminished use of the teeth and jaws, the masseter and temporal muscles have become reduced, and the sagittal and lambdoidal ridges have consequently became atrophied. The ecarinate rounded voluminous calvarium is the result.

It has been claimed that the young of higher species 'are constantly accelerating their development.' In many, however, development is retarded, inasmuch as infancy and juvenility are prolonged far beyond the periods observed in our simian relatives.

Such examples as this give cause to believe that the 'law of acceleration and retardation' has been at least unduly extended. Acceleration and retardation are, however, to a large extent, terms which express facts of evolution; whether the word law is applicable may depend on the meaning one gives the word.

The transmission of acquired characters was one of the accepted and most cherished dogmas of Cope, and the belief in transmissibility of such characters is an essential of the creed of so many who have become his followers in America that a special school came into existence known as the Neo-Lamarckian and also as the American School. My own prejudices have inclined me to that school. Nevertheless, when I have divested myself of such prejudices as well as I could, I have been compelled to admit that the evidence of the heredity of acquired characters was rather weak. There was, indeed, evidence for, a well as against, but that against the doctrine of the transmissibility of acquired characters seems to be the most weighty.

It is to be understood that the acquired characters considered in this connection are such as have been developed during postnatal life as a result of endeavors of the animal or of the influence of external agencies. The evidence presented has been mostly in support of the contention that the characters acquired have been directly inherited by offspring, and consequently the transition from the form not possessing the character to one having it is rapid. The evidence adduced has not been conclusive, to say the least. There is, apparently, a germ of truth in the proposition that acquired characters are transmitted, but in a modified sense, and the case has been weakened rather than strengthened by the evidence offered.

The evidence for inheritance of acquired characters was frequently given by Cope and in his last published work—'The Primary Factors of Organic Evolution,'—he marshalled the testimonies of many witnesses with his accustomed skill. He evoked 'evidence from embryology,' 'evidence from paleontology,' 'evidence from breeding;' he considered the 'characters due to nutrition,' 'characters due to exercise of function,' 'characters due to disease,' 'characters due to mutilation and injuries,' and 'characters due to regional influence'; he inquired into 'the conditions of inheritance,' and he fought against the 'objections to the doctrine of inheritance of acquired characters.' I have gone over all this evidence and yet I have not been convinced that the contention has been sustained that character acquired during the external life of an animal are transmitted. Many cases are alleged to sustain the 'inheritance of charac-

ters due to mutilation and injuries.' Some of these may be considered as mere coincidences; others provoke skepticism for one reason or other. To discuss them would be out of place here. But at least we may meet evidence with counter-evidence.

On the one hand, all the data and experiments recapitulated in the cases enumerated concern only two, or at most very few, generations of the animals in question, and were within the compass of a single man's life-time.

On the other hand, we have data and observations of the most reliable nature, and of an extraordinary compass. These have resulted not from experiments for the determination of a specific question, but from observances of a religious character. They were really in the nature of surgical operations, but for our purpose may be looked upon as experiments and have the value of contrived experiments. In no other field has such a series of disinterested experiments been available. They were conducted on countless millions of mankind and for thousands of years. The subjects experimented upon were kept isolated from others alike by their own prejudices and the prejudices of their neighbors. Circumcision is the term applied to the experiments in question.

For about 4,000 years circumcision has been practiced on a gigantic scale. Every male child among the Jews was operated upon, not only in Palestine, but wherever representatives of the race had wandered and adhered to their religion; religion itself was involved in the operation and it was regarded as a holy rite; the most scrupulous attention was paid to details. The operation was performed eight days after birth, and consequently there could be no functional activity of the tissues concerned. But after 4,000 years the newborn boys of the race come into the world with the special integument developed as much as in those of other races. Even the principle of atrophy through disuse has not become manifest in the case.

Other evidence, it seems to me, is the result of confounding the potentiality of a function with its manifestation. I allude to one set of examples on account of the interest of the cases, and I do so with the deference due to the eminence and ability of the gentleman who has furnished the evidence. That evidence has been collected under the head of 'inheritance of characters due to the exercise of function.' The evolution of the American trotting horse was considered. It was recorded that "by 1810 the taste for trotting as a sport had * * * increased here, and in 1818 it became a recognized sport under specific rules." * * * "At the end of 1824, six years after the first accepted three-minute record, the record had fallen to 2:34." * * * "By 1848 the record was lowered to 2:29½; the next decade lowered the record five seconds." Finally, at the close of 1895, the record had been furthered lowered to 2:08¼. * * * It is deduced from these premises that "there is nothing whatever in the actual phenomena observed anywhere along the line of this development of speed that would lead us to even suspect that the changes due to exercise of function had *not* been a factor in the evolution." But to me it seems that there is no evidence to show that the speed attained was other than would have resulted from taking the same animals untrained and then speeding the last. The speed is, of course, simply the expression of functional adaptation, and the horses were selected merely because, by their manifestation, they showed that they had the coordination of structural and psychological characters needed for the manifestation of the function. The manifestation guided the breeder to the selection of the animals. The successful animals were the pick of thousands unknown to fame.

But there is much in the history of the development of animals that seems to lead to the belief that eventually modifications may be due in part to acts of representatives of the phylum to which they belong. It is difficult to believe that some structural features are simply the result of natural selection operating on chance variations. An application of the doctrine of chances to some such cases appears to be adverse to the conception that they represent the influence of natural selection unaided.

A feature characteristic of most cave animals of widely diverse groups and classes is the atrophy of the eyes, and it seems to be most logical to attribute this to disuse of those organs in remote progenitors, and to assume that the atrophy may have resulted from a failure of nourishment by the nutrient fluid of the organs on account of the loss of functional activity rather than to selection by nature of forms with successively diminishing eyes. The presence of eyes in most cases certainly would scarcely be an element of disadvantage to animals, and it may be allowable to invoke some other agency than chance selection. We may be justified in postulating that the continuous disuse of the organs would in time react on the nutrition of the parts affected, and finally atrophy or disappearance would result. Like explanation would be applicable to the innumerable cases of atrophy of parts known to the naturalist.

But if cessation of nutrition culminates in final atrophy, increased nutrition of parts may result in hypertrophy and increased nutrition may be the concomitant of increased activity of parts. The exercise of such parts continued for many generations may react on the organization and the progeny at length be affected thereby. Of such cases Cope adduced many examples. The feet of the horse line furnish illustrations. The existing horse has the median toes and hoofs greatly hypertrophied and the lateral ones atrophied, but the remote ancestors had feet of nearly the same general pattern as the rhinoceroses and tapirs. Atrophy of the lateral digits has progressed inversely to hypertrophy of the middle ones. An analogous line of development culminating in feet superficially much like those of the horse was followed by another quite remote family of hoofed mammals, the Prototheriids of South America.

The idea of acceleration and retardation were associated by Cope with the idea that the course of evolution was determined from the beginning of things, and that life, to use his own words, is '*energy directed by sensibility or by a mechanism which has originated under the direction of sensibility.*' He maintained that 'consciousness as well as life preceded organism,' and he called this conception 'the hypothesis of archæsthetism.' This idea I refer to especially because it was broached in his vice-presidential address, delivered at the meeting of the American Association for the Advancement of Science in Philadelphia in 1884.*

I am myself unable to comprehend consciousness except as a product or result of organization, and those who wish to learn more about Cope's views respecting the question must refer to one of his many papers.

Whatever may be thought of Cope's philosophical views, his presentation of them is always interesting and some of them are illustrated with a wealth of facts that renders his communications valuable as repertories of well digested information. His first special paper, on 'The Origin of Genera,' published as early as 1868, is especially noteworthy for the mass of morphological data contained in it and for the apt manner in which they are tabulated.

VIII.

I venture to conclude with some reflec-

* Origin of Fittest, p. 425.

tions on the rank that may be assigned to Cope in the world of science.

Among those that have cultivated the same branches of science that he did—the study of the recent as well as the extinct Vertebrates—three naturalists have acquired unusual celebrity. Those are Cuvier, Owen and Huxley.

Cuvier excelled all of his time in the extent of his knowledge of the anatomical structure of animals and appreciation of morphological details, and first systematically applied them to and combined them with the remains of extinct Vertebrates, especially the mammals and reptiles. He was the real founder of Vertebrate paleontology.

Owen, a disciple of Cuvier, followed in his footsteps, and, with not unequal skill in reconstruction and with command of ampler materials, built largely on the structure that Cuvier had begun.

Huxley covered as wide a field as Cuvier and Owen, and likewise combined knowledge of the details of structure of the recent forms with acquaintance with the ancient ones. His actual investigations were, however, less in amount than those of either of his predecessors. He excelled in logical and forcible presentation of facts.

Cope covered a field as extensive as any of the three. His knowledge of structural details of all the classes of Vertebrates was probably more symmetrical than that of any of those with whom he is compared; his command of material was greater than that of any of the others; his industry was equal to Owen's; in the clearness of his conceptions he was equalled by Huxley alone; in the skill with which he weighed discovered facts, in the aptness of his presentation of those facts, and in the lucid methods by which the labor of the student was saved and the conception of the numerous propositions facilitated he was unequalled. His logical ability may have been less than that of Huxley and possibly of Cuvier. He has been much blamed on account of the constant changes of his views and because he was inconsistent. Unquestionably he did change his views very often. Doubtless some of those changes were necessitated by too great haste in formulation and too great rashness in publication. The freedom to change which he exercised, and which was exercised too little by at least one of his predecessors, was an offset to his rashness. He exercised a proper scientific spirit in refusing to be always consistent at the expense of truth.

His reputation at present is much inferior, at least among the people at large, to those of the men with whom he has been compared. Immediate reputation depends on various circumstances, some of which are quite adventitious, and it is often long before men find their true levels. It is scarcely premature to prophesy that Cope's reputation will grow and that in the future history of science his place will be at least as large as that of any of his predecessors.

THEO. GILL.

WASHINGTON.

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*EXPERT TESTIMONY.**

IT will be remembered that a would-be facetious barrister once remarked that prevaricators might be properly arranged in an ascending series, to wit: ordinary fibbers, liars and experts; an arrangement which I fear meets with the approval of many members of the bench and bar today. The cause for such harsh classification is not so very far to seek. It is based upon ignorance on the part of the bar, and at times upon what is worse than ignorance on the side of the 'expert.' With the culpable acts of the pseudo scientist we cannot waste our time. That he merits

* Address of the Vice-President and Chairman of Section C (Chemistry) at the Detroit meeting of the American Association for the Advancement of Science.

prompt condemnation is axiomatic, but a word is wanted touching upon what may be termed the ignorance of the Court.

"When I take my place upon the witness stand," said a prominent toxicologist once to me, "I can never predict in what shape I shall be upon leaving it;" a feeling with which most of us can, I fancy, sympathize pretty keenly.

Is it that we fear exposure of the weak points in our professional armor? Do we dread to say in public, "I do not know?" Hardly that, I take it. We are now possessed of so very little of that which one day may be known that no true scientist hesitates for an instant to plead legitimate ignorance. What really troubles us upon cross-examination is that the Court does not speak our language, a language often quite difficult of direct translation; that it is but rarely schooled in the principles of our science; and that, in consequence, it frequently insists upon categorical answers to the most impossible kind of questions.

The hypothetical questions showered upon the expert witness are sometimes veritable curiosities, so peculiar are they in their monstrosity. Who among us but has felt that the layman, who has simply to testify to observed facts, has an easy time of it, indeed, when compared with him from whom there is expected an opinion under oath?

All scientific men are willing and anxious to have their work scrutinized carefully by their peers; but to be exposed to the one-sided criticism frequently encountered at the bar is quite another matter; for it must be remembered that, after the adverse counsel has opened up what appears to be a glaring inconsistency in the testimony, the re-direct examination may utterly fail to repair the breach, because of a lack of familiarity with a technical subject on the part of the friendly attorney.

This leaves the witness in the uneviable position of disagreeing with the general drift of his own testimony, while it deprives him of suitable means of insisting upon its revision and correction.

According to the writer's view, there is but one way to escape such dilemma, and that is by direct and immediate appeal to the Judge, urging that the oath taken called for a statement of the whole truth, and not the misleading portion already elicited.

To illustrate how serious a matter the partial testimony of an expert witness may be, and to show also to what extent lawyers may go who look only to the winning of their causes, permit me to refer to an already reported poison case in which I was employed by the people. It may be roughly outlined as follows:

Much arsenic and a very little zinc were found in the stomach.

The body had not been embalmed, but cloths wrung out in an embalming fluid containing zinc and arsenic had been spread upon the face and chest.

Medical testimony showed that no fluid could have run down the throat. Knowing the relative proportions of zinc and arsenic in the embalming fluid, the quantity of arsenic found in the stomach was twelve times larger than it should have been to have balanced the zinc also there present, assuming them to have both come from the introduction of the said embalming fluid by cadaveric imbibition. Other circumstantial evidence was greatly against the prisoner.

At the time of my appearing for the people, on the occasion of the first trial of the case, my direct testimony brought out very strongly the fact that a fatal quantity of arsenic had been found in the stomach, but no opportunity was given me to testify to the presence of the zinc found there as well, although the fact of its existence in the body was known to the prose-

cution through my preliminary report. Through ignorance of the nature of such report on the part of the defence, no change was made in the character of the testimony during the cross-examination, and I was permitted to leave the witness stand with a portion of my story untold. No witnesses were called for the defence, and the case was given to the jury with the darkest of prospects for the prisoner.

For many reasons, unnecessary to recount here, I was distinctly of the opinion that murder had been committed, but I felt nevertheless that common justice demanded that the prisoner should have been entitled to whatever doubt could have been thrown upon the minds of the jury, no matter how far-fetched the foundations for such doubt might have been.

The first trial having resulted in a disagreement of the jury, I was pleased to learn, before the second hearing of the case began, that the defence was prepared to go into the question of the embalming fluid; for the responsibility of permitting only a part of what I knew to be drawn from me, to the entire exclusion of the remaining portion, was greater than I wished to assume. The nature of my report to the Coroner having been established, and certain opinions relating thereto having been fully ventilated, the jury were possessed of 'reasonable doubt' and acquitted the prisoner. What now were the duties of the expert upon the occasion of the first trial of this case and how should he have construed the meaning of his oath?

One eminent legal light, to whom the question was referred, held that the expert was distinctly the property of the side employing him, and that his duty was simply to answer truthfully the questions put to him, without attempting to enlighten the Court on facts known to him, but not brought out by the examination, no matter how vital such facts might be.

Another held that although the above course would be proper in a civil case, yet, in a matter involving life and death, the witness should insist upon the Court becoming acquainted with his whole story. Do not such differences in legal opinion make it very desirable that the expert, at least in capital cases, should be an employee of the bench rather than of the bar, in order that whatever scientific investigations are made may be entirely open to public knowledge and criticism?

Although the expert should earnestly strive to have what he has to say presented in the best form, he must remember that to secure clearness, particularly before a jury, technicalities should be reduced to a minimum. To a degree they are unavoidable, but let them be as few as possible. Illustrations should be homely and apt; capable of easy grasp by the jury's minds, and, if possible, taken from scenes familiar to the jury in their daily lives.

It is an unfortunate fact that the expert must be prepared to encounter in the court room not only unfamiliarity with his specialty, but also deep-rooted prejudices and popular notions hoary with age and not to be lightly removed from the mind by the words of a single witness. As President Jordan has well said, "There is no nonsense so unscientific that men called educated will not accept it as a science;" and, let me add, they will calmly attempt to shove the burden of proof upon the scientific man who is opposed to their views. Sanitary experts, in particular, run up against all sorts of popular superstitions and are inveighed against as 'professors' by those who consider themselves the 'practical' workers of the time; and, let it be noted, the burden of proof is uniformly laid upon these 'professors'' shoulders, while the most astounding and occult statements made by the 'practical' men may be received without verification.

One source of trouble, which perhaps is

peculiar to the water expert, lies in the impossibility of utilizing analytical results such as were made many years ago.

Those who are not chemists fail to grasp the fact that the examinaton of water may not be looked upon from the same point of view as the analysis of an iron ore. The statement that water analysis is of recent birth, and that it is yet in its infancy, is hard for them to appreciate, holding, as they naturally do, that what was true twenty years ago must be true to-day, if science does not lie.

A pit into which many an expert witness falls is prepared for him by insidious questions leading him to venture an opinion on matters outside of his specialty. It is a fatal error to attempt to know too much. Terse, clear answers, well within the narrow path leading to the point in question, are the only safe ones; and when the line of inquiry crosses into regions where the witness feels himself not truly an expert, his proper course is to refuse to testify outside of the boundaries of his legitimate province.

Unfortunately, the expert is as often invited to take these collateral flights by the side employing him as by the opposition. Affidavits in submitted cases are commonly written by the lawyers and not by the expert, although they are, of course, based upon his reports. In the strength of his desire to win the case, the lawyer often prepares a much stronger affidavit than his witness is willing to swear to.

The writer has had no little difficulty just at this point, and has had plenty of occasion to observe the irritation displayed by counsel upon a refusal to endorse statements which have been 'too much expanded.'

Every expert witness, especially in his early cases, is sure to have adverse authorities quoted against him; therefore it behooves him to be so familiar with the literature of his subject as to be capable of pointing out that such and such a writer is not up to date, or that such and such a passage, if quoted in full, would not bear the adverse construction that its partial presentation carries. When the expert reaches a position of such prominence that he can state a thing to be so because he says it, irrespective of whatever may be written on the subject to the contrary, his course then is greatly simplified; but long before he attains that altitude he will have put himself upon record in many cases, and happy for him if the record so made be such as cannot be quoted to his disadvantage.

"If I had only not written my first book," is the reflection of many a distinguished author; while one of the great masters of music, referring to an opera, said: "It is one of my early crimes."

Above all things, the expert should "provide things honest in the sight of all men."

It is well for him to be deeply interested in his case, to feel in a measure as if it were his own, but it is unwise in him to become so partisan as to let his feelings affect his good judgment, and it would be indeed criminal should he permit his interest in any way to contort the facts.

Before the case is brought to a final hearing, it may be apparent that experiments before the Court are possible and they may be demanded by the counsel in charge of the case. If such experiments be striking, easy of execution, and not too long, by all means make them.

Practical illustrations, particularly such as involve some fundamental principle, have great weight with the Court; but these illustrations must not be such as would turn the court room into a temporary laboratory and involve the loss of much time in vexatious waitings.

Such experiments as are determined upon should be thoroughly rehearsed beforehand, no matter how simple they may be; for, of

all failures, the court-room experiment which declines to 'go off' is perhaps the most dismal.

This brings to mind a kindred topic upon which there should be a word of caution; laboratory experiments which work to perfection may utterly fail when expanded to commercial proportions, so that it is wise to bear in mind the danger of swearing too positively as to what will happen in large plants, when the opinion is based only upon what is observed to occur upon the smaller scale. Like conditions will, of course, produce like results, but it is marvellous how insidiously unlooked-for conditions will at times creep into one's calculations, and how hard it is even to recognize their presence.

When preparing his case for presentation, the expert often errs in not dwelling more largely upon certain points because he thinks them already old and well known. To him they may be old, but to the public they may be of the newest. Not only is the public unequally posted with the specialist, but what it once knew upon the subject may have been forgotten. Is is well, therefore, to insert, in a special report, matters that would be properly omitted from a paper prepared for a professional audience.

Sanitary problems are of especial interest to the public, but the amount of ignorance, or rather false knowledge, displayed concerning them is surprising and often difficult to combat. The sanitarian is not unfrequently called upon suddenly to defend a position involving complex statistics; and, because the data cannot be forthwith produced, the inference is drawn that his points are really without facts to support them and that they are consequently not well taken.

Long before he gets into Court, particularly if the time for preparation of the case be short, the expert may well 'pray to be delivered from his friends.' He may receive a peremptory order by telegraph to 'determine the mineral qualities of this rock,' when the telegram should have read 'Assay this ore for silver;' and later it may be a matter of surprise that a quantitative knowledge of the copper present was not obtained while passing along the line for the determination of the silver; for it is generally not known that the complete analysis of anything is quite rare and correspondingly tedious and expensive.

Toxicologists who hear me may call to mind some case involving a search for the presence of an alkaloid, strychnia for example, during which search the District Attorney, in his eagerness for information may have asked to know what the indications were as to the presence of the poison, at a time when the extraneous organic matter was not nearly removed. He may have wished no final report, but only the simple probabilities, whereon to base a possible arrest. Such requests are very common, and are akin to a demand for a proof of the pudding during the early baking, when we all know that such proof comes at a much later stage of the proceedings.

Finally, "When doctors disagree who shall decide?"

This question is often very vigorously settled by the jury, as was instanced in a recent celebrated murder trial in New York City. In that case what the experts had to say on either side was simply thrown overboard as a whole, and the finding was based upon the testimony of the remaining witnesses.

What can be said upon this question of the disagreement of expert witnesses? First, it must be noted, they are far from being the only class of people who fail to agree, and that too on very important subjects. Do my hearers think it would be a very difficult task to find a small army of men who would testify very variously and very posi-

tively upon questions of politics or religion? Would it be hard to find 'good men and true' who would give under oath greatly differing opinions concerning the propriety of instituting free trade or establishing an inheritance tax? Experts are subject to the same errors of judgment as befall the rest of professional humanity, and when their opinions clash they are entitled to the same respect that we grant to the members of the bench when they hand down the decision of a divided Court.

One fruitful opportunity for disagreement always arises when questions are brought into Court touching upon matters newly discovered and apart from the well beaten path of common professional knowledge. Doubt is often left upon the minds of those seeking the light, even when the testimony is given by the specialist who originally developed the new point in question, for one cannot be expected to be thoroughly educated in that which he has himself but recently discovered.

Many of us have dreaded to see the 'ptomaines,' or putrefactive alkaloids, make their way into Court with their mystifying influences upon Judge and jury and their tendency to protect crime. Now they are in, what is to be the end? Even with no 'Ptomaine theory' possible, the ptomaine form of argument is not unknown. The writer was once asked in an arsenic case whether he was willing to swear that at some future time an element would not be discovered giving the stated reactions now called arsenical. Such nonsense is, of course, instituted to impress the jury, and is suggested by similar questioning in the alkaloid cases.

A recent and somewhat amusing instance arose from an attempt to introduce the rather new conception of 'degeneracy' into a murder trial. The defence sought to show that the prisoner was a 'degenerate' and offered expert testimony as to the meaning of the term and as to the signs whereby such a condition was to be recognized, whereupon the prosecution called attention to the fact that the defendant's experts themselves exhibited every one of the signs in question.

Having said all that he was to say, and having stated it to the best advantage, should the expert depend upon the stenographer so recording it as to allow of its being used in future without correction? Decidedly not.

The average stenographer is unfamiliar with technical terms, especially such as are chemical, and the witness who fails to supervise the minutes may find out later that he has sworn to a most remarkable array of 'facts.' The writer once discovered that he had recommended, as a very efficient method of purifying a city water, the filtering of the entire supply 'through a layer of black mud.' Not to take your time further, let us summarize what has thus been briefly said:

The expert witness should be absolutely truthful, of course; that is assumed, but beyond that he should be clear and terse in his statements, homely and apt in his illustrations, incapable of being led beyond the field in which he is truly an expert, and as fearless of legitimate ignorance as he is fearful of illegitimate knowledge.

Mounting the witness-stand with these principles as his guide, he may be assured of stepping down again at the close of his testimony with credit to himself and to the profession he has chosen.

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CURRENT NOTES ON ANTHROPOLOGY.

ARAUCANIAN STUDIES.

Dr. Rodolfo Lenz continues his excellent studies on the Araucanian dialects and folk-lore, in the 'Anales de la Universidad

de Chile.' His last contribution embraces seven semi-mythical tales in the Pehuenche dialect, the original text and a Spanish translation. They offer much curious material, and often leave it doubtful whether the story is of native origin or borrowed from European sources. The first, for example, tells of a dead lover who came from his grave to claim his bride and carried her to his tomb. In spite of the striking similarity of this to the legend embodied in Bürger's ballad 'Lenore,' the editor believes it to have been from native sources.

Unfortunately, like so many other tribes, the Araucanians were little studied by the early settlers, and the knowledge we have of their mythology is vague and slight. Dr. Lenz very properly observes that it is all the more important to collect what still survives in their songs and stories; and, it may be added, the scholarly manner in which he presents his researches to the reader renders them models of work of this kind.

THE 14TH REPORT OF THE BUREAU OF ETHNOLOGY.

THIS report (for 1892–93) has just been distributed. It is in two parts or volumes counting up to over twelve hundred pages! The contributions are three in number, the first an exceedingly interesting paper by Mr. James Mooney on the ghost dances of our Western tribes; the second a study of the Menomini Indians, by Dr. Walter J. Hofman, containing a mass of accurate observations; and the third an erudite treatise on the expedition of Coronado to New Mexico in 1540, by Mr. George Parker Winship.

It is needless to dwell on the value of these contributions to the history and ethnography of our country. Every future student of these subjects will owe a debt to this and previous reports of the Bureau. No series of publications by our government has been edited with more conscientious care, and none can show a list of articles of a higher class, or of more permanent importance, than the Bureau of Ethnology. It should be a matter of patriotic pride, based on the recognition of solid merit, for the government to render liberal aid to this scientific department and increase the means of its usefulness.

D. G. BRINTON.
UNIVERSITY OF PENNSYLVANIA.

NOTES ON INORGANIC CHEMISTRY.

A LECTURE by Professor William Crookes on 'Diamonds' was delivered June 11, 1897, at the Royal Institution. It has been reprinted in the *Chemical News* and is perhaps the best brief treatise on the diamond ever written. The latter part of the lecture was devoted to the origin of the diamond as illustrated by the diamond 'pipes' of the Kimberley field. According to Professor Crookes the diamonds crystallized out of molten iron containing carbon in solution and at sufficient depth below the surface to give great pressure. Water finding its way down to this iron, the gas generated bored out the 'pipes' which were, "at the subsidence of the great rush, filled with a water-borne magma in which rocks, minerals, iron oxid, shale, petroleum and diamonds are churned together in a veritable witch's cauldron," a mud volcano. "It may be that each volcanic pipe"—of the South African fields—"is the vent for its own special laboratory—a laboratory buried at vastly greater depths than we have reached or are likely to reach—where the temperature is comparable with that of the electric furnace; where the pressure is fiercer than in our puny laboratories and the melting point higher; where no oxygen is present, and where masses of carbon-saturated iron have taken centuries, perhaps thousands of years, to cool to the solidifying point. Such

being the conditions, the wonder is, not that diamonds are found as big as one's fist, but that they are not found as big as one's head. The chemist arduously manufactures infinitesimal diamonds, valueless as ornamental gems; but Nature, with unlimited temperature, inconceivable pressure and gigantic material, to say nothing of measureless time, produces without stint the dazzling, radiant, beautiful crystals I am enabled to show you to-night."

Professor Mourelo, of Madrid, has investigated the preparation of a strongly phosphorescent strontium sulfid. The pure compound shows no phosphorescence, but the presence of a small quantity of alkali seems necessary, and a trace of subnitrate of bismuth is of advantage. When the mass which has been strongly heated is very slowly cooled, it shows after the action of even very little light, a strong phosphorescence. This property is lost on pulverization, but may be restored by long heating with starch.

In the *Zeitschrift für Angewandte Chemie* Th. Bokorny gives the results of a study of the antiseptic action of various substances. A culture medium of half per cent. egg albumen or peptone, with one-tenth per cent. potassium phosphate, two-tenths per cent. magnesium sulfate and a trace of calcium chlorid was infected with the bacteria of decay, and after addition of the substance to be tested, placed for several days in an incubator. Among inorganic compounds silver nitrate and mercuric chlorid have about the same value, 0.002 %, killing all organisms in two days. The antiseptic limit with silver nitrate is 0.0002 %; with mercuric chlorid 0.001 to 0.0002 %. Copper sulfate is nearly as active, 0.005 % killing all organisms in twenty-four hours, and 0.001 % preventing decomposition. Zinc sulfate 0.01 % kills infusoria in eighteen hours, but 0.1 % is not completely antiseptic, while cadmium sulfate toward algæ and infusoria is weaker than the zinc salt, but toward bateria stronger, 0.02 % being antiseptic. Lead acetate and nitrate in 0.1 % solution only delay decay, while it is prevented by the same strength of iron sulfate solution. The fluorids are not strong antiseptics, the limits being for hydrofluoric acid 0.02 %, barium fluorid 0.3 %, aluminum fluorid 0.1 %, calcium fluorid 0.03 %, ferric fluorid 0.06 %, magnesium fluorid 0.05 %. Ammonium fluorid 0.1 % is without action, but sodium fluorid 0.1 % is antiseptic; potassium fluorid is rather more active. J. L. H.

NOTES ON ENGINEERING.

A committee of the British Institution of Civil Engineers, appointed a year ago or more, have reported the following recommendations on steam-engine efficiency, and they have been adopted by the Council:

(1) That the statement of the economy of a steam-engine in terms of pounds of feed-water per I.HP. per hour is undesirable.

(2) That for all purposes except those of a scientific nature it is desirable to state the economy of a steam-engine in terms of the thermal units required per I.HP. per hour (or per minute), and that if possible the thermal units required per brake HP. should also be given.

(3) That for scientific purposes the thermal units that would be required by a perfect steam-engine working under the same conditions as the actual engine should also be stated.

The proposed method of statement is applicable to engines using superheated steam as well as to those using saturated steam, and the objection to the use of pounds of feed-water, which contain more or less thermal units according to conditions, is obviated, while there is no more practical difficulty in obtaining the thermal units per I.HP. per hour than there is in arriving at the pounds of feed-water.

For scientific purposes the difference in the thermal units per I.HP. required by the perfect steam-engine and by the actual engine shows the loss due to imperfections in the actual engine.

A further great advantage of the proposal is that the ambiguous term 'efficiency' is not required.

In the contest which has now been so long

waged over the disputed originality of invention of the high explosive 'cordite,' Mr. Hiram Maxim, one of the contestants, and one in whom his fellow-countrymen in the United States feel much interest, is, for the time at least, defeated. Referring to this important case, one of the English technical journals, *Industries and Iron*, makes the following remarks: "The appeal of the Maxim-Nordenfelt Guns and Ammunition Company against the judgment delivered by Mr. Justice Wright in the Cordite case has been, as was generally anticipated would prove to be the case, against the appellants. The judgment of the Court was couched in somewhat uncompromising language, and it is doubtful whether the Maxim-Nordenfelt Company will consider it advantageous to carry the matter any further. Although the Court is no doubt perfectly right in its definition of the nature of the invention comprised in Mr. Maxim's patent, as against that of Sir Frederick Abel and Professor Dewar, none can deny the fact that Mr. Maxim was the undoubted pioneer in smokeless powders as we now know them, and it will be conceded by most that the recent decision involves a certain degree of hardship. Mr. Maxim's invention of a smokeless powder was not an accidental discovery; he was forced into the course of the investigation he took up by the necessity of procuring a powder which would be suitable for the rapid-firing gun which he had invented. He found that the use of the ordinary powder practically destroyed the utility of his gun, by surrounding it with such clouds of smoke that it was impossible to take aim. Mr. Maxim then deliberately set himself to the purpose of producing a powder which should be almost, if not altogether, smokeless. In this he was absolutely successful, and he has only shared the fate of many other inventors in seeing the fruits of his invention taken away from him through legal technicalities."

A FOREIGN exchange makes the following very unequivocal statement relative to our later systems of procedure in the Patent Office. A recent change in the law prevents any such delay, whether intended or otherwise on the part either of the holders of the patent or of the officer of the Patent Office: "The amazing circumstances which characterized the issue of the Berliner patent, to which is due the controlling interest in the United States of the Bell Telephone Company, have been paralleled by the well-known Bradley patents, which are now creating a good deal of stir among the manufacturers of aluminium and carbide of calcium. In the former case, the patent lay in the Patent Office for a period of no less than thirteen years before it was formally issued to the public. The Bradley patents were similarly interred for nine years before they were resurrected for the purpose of being used against the Pittsburg Reduction Company. It must be a gratifying reflection to those interested in patents and inventions in the United States that the singular course of procedure suggested by the foregoing instances will not be allowed to characterize the American Patent Office much longer. On January next the new regulations come into force, by which it is enacted that the issue of a patent shall be compulsory within a certain definite period from the date of application; and, under these circumstances, it seems likely that the familiar legend 'Patent applied for' on American manufactures will shortly become effete."

R. H. T.

SCIENTIFIC NOTES AND NEWS.

IN view of the International Congresses of Geology and Medicine, meeting in Moscow during the present month, the tenth meeting of Russian men of science and physicians, which was to have been held at Kief during the same month, has been postponed until 1898.

THE President of the local executive commit-

tee of the British Association for the Advancement of Science announces that McMaster University, a college residence, has been selected as the headquarters of the American Physiological Society during the Toronto meeting of the British Association. Rooms and board may be obtained there at $1.50 to $2.00 per day.

The French Association for the Advancement of Science will meet next year at Nantes. At the meeting being held this week at St. Étienne the public lecture was to have been given by M. Gariel, on the Röntgen rays. Subjects proposed for special discussion were: Section of Physics, 'Atmospheric Electricity;' Section of Meteorology, 'The Study of Clouds;' Section of Geology, 'The Formation of the Fossiliferous Basins of the Central Plateau;' Section of Hygiene, 'The Part played by Leaves of Absence from School in the Spread of Contagious Diseases.'

A meeting of the Council of the Australasian Association for the Advancement of Science was held at the Royal Society's House, Sydney, on June 17th, with fifteen members in attendance. Letters were read from the Royal Society of Tasmania, the Melbourne Branch of the Royal Geographical Society and the Medical Society of Queensland, suggesting a memorial to the late Baron von Müller. A preliminary committee was appointed to make arrangements for the meeting to be held at Sydney in January next. A large number of papers have been promised for the several sections; Section G., Economic Science and Agriculture, leading with twenty-eight papers, followed by Section F., Ethnology and Anthropology, and Section J., Mental Science and Education, each with eighteen papers.

James Hammond Trumbull, LL.D., the well-known philologist, died in Hartford, Conn., August 5th, aged seventy-six years. For many years he paid especial attention to the subject of the Indian languages of North America. He was a member of the National Academy of Sciences and of many other learned societies.

We regret also to record the death of Captain Bertram Lutley Sclater, son of the eminent zoologist, who died at Zanzibar on July 24th, at the age of thirty-one years, of fever contracted while making surveys and explorations in British Central Africa; of Dr. Golowkinskie, formerly professor of mineralogy and geology in the Universities of Kasan and Odessa, at Kastel, on June 9th, and of M. Étienne Vacherot, formerly professor of philosophy and assistant to Cousin, and the author of numerous contributions to philosophy, political economy and science, at the age of eighty-six years. M. Vacherot took a prominent part in French politics, having been one of the Mayors of Paris during the siege.

Dr. Wilhelm Thierry Preyer, whose death we recently noticed, was a man of unusual versatility and originality. He was born in Manchester, England, in 1841, and received his education in that country until he was sixteen years old. After studying at various German universities and at Paris, he qualified as docent in the University of Bonn, first in zoology and afterwards in physiology. His first work was in zoology, and he published several books treating of problems in which zoology and physiology are both concerned, such as 'The Struggle for Existence' (1869), 'Hypotheses regarding the Origin of Life' (1875), and 'Spontaneous Generation and the Conception of Life' (1879). In 1869 Preyer was made professor of physiology at Jena, but removed to Berlin in 1880 and qualified as docent in the University. He was not promoted to a professorship at Berlin and removed to Wiesbaden in 1893. In addition to the works mentioned above, Preyer wrote numerous articles and at least two volumes concerning the general problems of science, partly of a popular character. He published an 'Elements of General Physiology' in 1883 and had earlier published a book on the 'Five Senses of Man,' translated into English in the International Scientific Series. He also made many contributions to the physiology of the senses and to experimental psychology, and was one of the Board of Editors of the *Zeitschrift für Psychologie*. His volume on 'The Child's Mind' (1879) has been the starting point of numerous similar observations, though it is interesting to note that the first scientific biography of a child was written by Darwin. Preyer's versatility is

further illustrated by the fact that his last volume was on graphology.

THE statue of Darwin at the entrance to the public library and museum of Shrewsbury, to the erection of which, by the Shropshire Horticultural Society, we have already called attention, was to have been unveiled on August 10th. The statue, in bronze, is by Mr. Horace Montford.

ON the eleventh of July a monument was unveiled at Bresches to Velpeau, the eminent French surgeon.

A MEDALLION portrait of Pasteur, by M. Patey, after the medal by M. Roty, has now been placed on the wall of the École Normale Supérieure, the site of the former laboratory of Pasteur. The inscription previously inscribed on the tablet reads:

ICI FUT LE LABORATOIRE DE PASTEUR.
1857.—Fermentations.
1860.—Générations spontanées.
1865.—Maladies des vins et des bières.
1868.—Maladies des vers à soie.
1881.—Virus et vaccins.
1885.—Prophylaxie de la rage.

THE position of Assistant Chief of Division of Soils, Department of Agriculture, salary $1,800 per annum, is to be filled by the Civil Service Commission. Applicants must submit, not later than September 1st, original essays, either printed or in manuscript form, consisting of not less than 5,000 words, and containing a thorough treatment of the subject, 'Environment as affecting the yield, quality and time of ripening of crops.'

DR. JOHANNES MARTIN has been appointed Director of the Natural History Museum of Oldenburg, and Dr. Philippi, assistant in the Natural History Museum in Berlin.

THE Prince of Wales has been elected a fellow of the Royal College of Physicians, London, or, as the English papers put it, he has complimented the profession by accepting the fellowship. As a non-medical fellow, the Prince has only three predecessors, the Marquis of Dorchester in 1658, the Duke of Manchester in 1717, and the Duke of Richmond in 1729.

THE Legislature of Uruguay has conferred honorary citizenship and the sum of $10,000 on Dr. Sanarelli as a recognition of his discovery of the yellow fever microbe.

THE Faculty of Philosophy of the University of Strasburg has conferred the honorary degree of Doctor of Philosophy upon Professor Flinders Petrie, the celebrated Egyptologist.

AT the annual general meeting of the British Medical Association, on July 27th, the gold medals for distinguished merit were awarded to Sir Walter Foster and Mr. C. G. Wheelhouse. The Stewart prize was awarded to Dr. G. S. Woodhead, and the Middlemore prize to Dr. Alexander Hill Griffith.

THE Yerkes Observatory of the University of Chicago will be dedicated on October 1st. The program is not yet announced, but it is planned to hold a series of conferences as part of the ceremonies, and it is hoped that a large number of astronomers and other men of science will be present.

REPLYING recently to a committee asking for the earlier opening of Kew Gardens, Mr. Akers-Douglas said the *raison d'être* of the existence of Kew Gardens was the valuable scientific work it did, and he could not be expected to do anything in the way of extending the hours during which the Gardens were open to the general public if it would interfere with that work. The financial question did not weigh with him at all, for if he were convinced that the interests of science would not suffer by the earlier opening he should endeavor to persuade the Treasury to grant any extra money required. The sole question for consideration was whether the interests of science could be combined with the desire of the people for the earlier opening, and he regretted to say that the scientific men whose opinions he had obtained were entirely opposed to the proposal. From a scientific point of view the experiment had not been a success in Edinburgh, and they had no reason to anticipate any better result at Kew.

WE have published several notices of the expedition in Central Asia under the direction of the Swedish traveler, Dr. Sven Hedin. He is now preparing the results of his explorations for publication, and is expected to present these before the Royal Geographical Society of London

early in the autumn. The Russian Geographical Society will confer its gold medal upon him.

It is stated in *Natural Science* that under the directorship of Dr. T. Kochibe, the Geological Survey of Japan has been making good progress, and the staff has been increased. There has for some time been accumulating a collection chiefly illustrative of practical geology, and it is now proposed to build a proper geological museum in Tokyo. A short time ago some valuable phosphatic beds of Tertiary age were discovered along the northeast shore of the province of Kyushu, and Dr. Tsuneto, of the Agronomic division of the Survey, has been experimenting with the material so as to make it available for the small Japanese peasant farmers to use as manure. The organic remains in the deposit are those of marine invertebrates.

Dr. W. L. Abbot, of Philadelphia, who has made valuable donations to the Smithsonian Institution, has now given a collection of birds from the Malayan peninsula containing 1,100 birds representing over 200 species.

Stanford University has been given by the brother of the late Senator Stanford, now residing in Australia, a collection of books relating to Australia, including especially works on the geography, geology and anthropology. It consists of 2,500 bound volumes and 3,000 pamphlets.

Mr. Fortesque Flannery, M. P., has announced his intention to move in the House of Commons: "That in the opinion of the House it is desirable that a department of public health be constituted and that the same be under the charge of a responsible Minister having a seat in Parliament."

A committee, consisting of Lord Crawford, Sir Edward Thompson, Sir Benjamin Stone, Professor Meldola, Captain Abney and others, has been appointed for the purpose of forming a National Photographic Record Association, the object of which is to be the collecting and making photographic historical records of the British Isles.

An International Congress is being arranged at Paris for the discussion of the means of preventing fires in theatres and other places of public resort.

The New York *Evening Post* states that an extensive robbery of art works and other articles of great value has taken place at the museum of the Canton of Vaud. The museum is located in the College near the Cathedral of Lausanne, and contains natural-history collections from Aventicum and Vidy, the ancient Lausanne, and interesting antiquities from lake-dwellings, coins, medals, etc. It also contains the Cantonal Library of 60,000 volumes.

A curious robbery has occurred in Paris, a number of rabbits inoculated with the germs of diphtheria, cholera, typhus, etc., having been stolen from the Aubervilliers Hospital. These are thought to have been sold to dealers, and there was a general panic in Paris among those who had eaten rabbit!

The British Home Office will only allow five pounds of carbide of calcium to be kept without a license. It must then be kept in separate, substantial, hermetrically closed metal vessels containing not more than one pound each.

The New York State Commission on Voting Machines, consisting of Philip T. Dodge, Professor R. H. Thurston and Mr. H. de B. Parsons, held their first meeting at Albany on July 10th, for the examination of machines to be adopted by the State.

Arrangements have been made in Boston to give members of the fire department instruction in the nature of electricity and the uses of electrical appliances.

A new fire-proof building will be erected for the collections of the Pathological Institute at the University at Berlin.

At its meeting in Washington in May, 1897, the American Physiological Society appointed a committee to consider whether the time had come for the publication of an American Journal of Physiology. The committee have reported in favor of the plan and it will undoubtedly be adopted at the next meeting of the Society. The journal, which will be devoted to investigations in physiology and allied sciences, will probably begin publication in January next and will be edited by a committee of the Society, with Dr. W. T. Porter, Harvard Medical School, as Secretary.

The *American Naturalist* has been purchased

from the estate of the late Professor Edward D. Cope and will, beginning with the next number, be edited by Dr. Robert P. Bigelow, instructor in biology in the Massachusetts Institute of Technology, assisted by an editorial committee and a board of associate editors whose names are not yet announced.

THE Zoological Society of Tokyo has begun the publication of *Annotationes Zoologicæ Japonensis*, under the editorship of Professor K. Mitsukuri, intended for contributions shorter than those contained in the Journal of the University. Professor Mitsukuri contributes an interesting introduction on the development of zoology in Japan, making due acknowledgment of the great services of Professors Morse and Whitman.

WE have received the first number of the second volume of the Bulletin of the Pará Museum of Natural History and Ethnology. The number, published in May of the present year, is largely devoted to the somewhat belated report of the director, Dr. Emilis A. Goldi, for 1895. There are attached to the museum a Zoological Garden and a Botanical Garden, a Library and a Meteorological Bureau. Numerous additions to these various institutions are reported during the year, the annual appropriation for the museum in 1895 being $70,000, and $12,000 each for the zoological and botanical gardens. The museum is visited daily by between 500 and 600 persons. The Bulletin further contains an account of an expedition, chiefly for archæological research on the Rio Maracá by A. P. de Lima Guedes, and four scientific papers.

THE manuscripts of the late Julius Sachs, the eminent botanist, will be edited by Professor Noll, of Bonn.

D. APPLETON & CO. announce that they will have ready for publication in the autumn the biography of Huxley prepared by his son.

LONGMANS, GREEN & CO. will shortly publish a new edition of 'Ancient Stone Implements, Weapons, and Ornaments of Great Britain' by Sir John Evans, President of the British Association. The work will be thoroughly revised, as much new material has been discovered since the first edition was published in 1872.

THE Civil Tribunal at Paris dismissed, on July 22d, the claim for 5,000f. damages preferred by the publisher of the French translation of Dr. Nansen's book against the *Correspondant*, in which Mme. Marie Dronsart reviewed Dr. Nansen's career and gave many extracts from the English version of his book.

PROFESSOR R. LYDEKKER contributes to the issue of *Nature* for July 15th an extended article entitled 'Species and Subspecies,' based upon the articles contributed to this JOURNAL by the Hon. Theodore Roosevelt and Dr. C. Hart Merriam. Professor Lydekker concludes his article with the following suggestion: "The question of the distinction between species and subspecies is undoubtedly one bristling with difficulties, and it is therefore one which in many cases is incapable of being definitely settled by an individual opinion. Although personally convinced of the advisability of using specific names in a wide sense, and employing trinomials for the designation of the nearly related forms, it may be suggested that an international committee of zoologists should be formed to discuss the question in all its bearings. Needless to say, such a committee should include representatives of both the 'splitting' and 'lumping' interests; and if the points at issue were fairly debated, with a full determination to give and take on both sides, it is difficult to believe that a working compromise between the extreme views could not be arranged. Almost anything is better than the present condition of uncertainty and discrepancy."

THE Plague Commission sent by the Austrian government to Bombay to study the disease has presented a provisional report to the Vienna Academy of Science. According to the *British Medical Journal* the Commissioners state that they had opportunities of investigating the plague clinically in 70 cases, and that they had opportunities of making pathological and bacteriological investigations on 47 bodies of persons who had died of the disease. Pathologically they say that three forms of plague can be distinguished: a septicæmic-hæmorrhagic

form, in which the whole lymphatic apparatus appears to be diseased in a peculiar manner; a septico-pyæmic form, with metastases to internal organs; and primary plague-pneumonia, a lobular pneumonia with quite characteristic appearances. The mode of entrance of the infection seems mostly to be the skin, more rarely the lungs and the tonsils, never the intestinal canal. The Yersin-Kitasato bacillus is certainly the exciting cause of the disease; it can be obtained pure from the organs as well as from the blood. The Commissioners were unable to satisfy themselves that Haffkine's serum injections had any effect. They considered it improbable that the plague could find a footing in Europe.

THE commission on the plague under the direction of Professor Koch has also issued its report. It states, according to the London *Times*, that the plague bacillus outside the human body or certain animals has very brief vitality. Pure cultures with which experiments were made were killed by sublimates at boiling temperature immediately, by mineral acids in five minutes, by a solution of 1 per cent. of carbolic acid in ten minutes, and by milk of lime exposed to sunlight in one hour. The duration of the life of the bacillus was found to be from eight to ten days at the most. Rats were found to be in the highest degree susceptible and to be spreading the plague germs and communicating them to human beings. For experiments on immunity Yersin serum was used with apes. Its protective power in the case of brown apes did not exceed eight days. Strong injections of serum proved to be of unquestionable curative efficacy. Haffkine's system of inoculation, which was applied to 1,400 patients, is said, contrary to the report of the Austrian Commission quoted above, to have showed undoubted protective results, although a number of the patients were taken ill in consequence of the inoculation.

REGULATIONS have been issued by the German government for the sale of Professor Koch's new tuberculin, under which name the new specific will be sold by chemists in phials containing one millilitre at Marks 8.50 and in phials containing five millilitres at Marks 42.50. The tuberculin will only be given to certified medical men or to those provided with an authorization from such.

A RESOLUTION has been submitted to the municipal council of Paris requiring families to furnish every two months a medical certificate stating that infants under one year have been cared for in accordance with hygienic rules.

UNIVERSITY AND EDUCATIONAL NEWS.

THE full text of the government bill with respect to the University of London is published in the issue of the *Times* for July 24th. The Commissioners are Lord Davey, Dr. Mandell, Lord Bishop of London, Lord Lister, Sir William Roberts, M.D., Sir Owen Roberts, Professor Jebb and Mr. E. H. Busk. The Commissioners are required to make statutes and regulations for the University of London in general accordance with the recommendations of the Cowper Commission. The Commissioners are to be superseded at the end of 1898 by a Senate of the University consisting of the Chancellor and other representative members. The Senate has entire conduct of the University, it only being provided that:

(*a*) No religious test shall be adopted, and no applicant for a University appointment shall be at any disadvantage on the ground of religious opinions;

(*b*) No procedure to a higher degree shall be allowed without examination or other adequate test, nor shall any honorary or *ad eundem* degree be conferred unless the Senate, in exceptional cases, think fit to confer such a degree on a teacher of the University;

(*c*) No disability shall be imposed on the ground of sex.

A LAW passed by the last Legislature of the State of Illinois appropriated to the State University the sum of $456,000, lost to the University by the defalcation of the former Treasurer.

THE faculty of sciences of the University of Paris has been authorized to give a certificate for higher studies in physical geography.

A NEW technical school at Northwich, built by Sir Joseph Verdin at a cost of £12,000, was formerly opened on July 24th.

IT is proposed to establish at Shanghai a university for the education of the Chinese in Western art, science and literature.

DR. R. S. CURTIS, of the University of Chicago, has been elected professor of chemistry in Hobart College.

MR. JOHN P. HYLAN has been appointed instructor in experimental psychology in the University of Illinois.

DR. HOPE, lecturer on hygiene at University College, Liverpool, has been made professor.

DR. TRAUBE, Privatdocent at Berlin, has been appointed to the newly established professorship in the Technological Institute at Charlottenburg, and Dr. Adalbert Kolb, Privatdocent in chemistry in the Technological Institute at Darmstadt, has been promoted to a professorship.

DISCUSSION AND CORRESPONDENCE.

CEREBRAL LIGHT.

IN SCIENCE for July 23d, p. 138, I find a letter from Dr. Scripture in which he makes some very acute observations on the the origin of the figures, usually irregular and obscure, but sometimes quite definite, which are seen in the dark field of the closed eyes. In past years I have spent many hours in studying these figures and they are briefly described in my little volume on Sight, pp. 66 and 67 of last edition. They are usually considered as of retinal origin and sometimes spoken of as 'retinal light;' but Dr. Scripture gives what he thinks conclusive reasons for thinking that they are of cerebral origin, and therefore proposes the name 'cerebral light.' Now, as to the question of origin, I am not prepared to say anything. I wish now only to show that his supposed tests are not valid.

1. He says that with the eyes closed there is but *one* dark field, instead of two, as there ought to be if its origin is retinal; for there are two retinæ. Now, if he means the simple field without reference to the figures in it, I would ask: How could there be more than one? Even with the eyes open, there seems to be but one field. Only by close observation can we see that there are really two partly overlapping fields forming a common field bounded on the two sides by the faint images of the nose. But in the dark field there are no images of the nose. But if, on the other hand, he means that the two retinæ could not be expected to be similarly affected in all parts, and therefore there ought to be different figures for the two retinæ in the same dark field, then I would ask again: How are we to distinguish the figures belonging to each retina in the one dark field?

2. But, in further proof, he says: These figures do *not move* with the movements of the eye; while after-images, which are admittedly retinal affections, do thus move. Now, I find, on the contrary, that these figures behave exactly as the after-images do. I find that, in looking in a different direction in the dark field, they may indeed disappear, but only to reappear at the new point of sight. After-images do the same. Unless they are very strong, they also, on changing the point of sight, disappear to reappear at the new point.

It is possible, however, that we are talking about different things. It is possible that there are two different kinds of figures in the dark field, one retinal and the other cerebral.

3. But, again, he says that these figures do not change place when the axis of the eye is displaced by pressure in the corner, whereas after-images do change place under these conditions. Now, on the contrary, I find that after-images under these conditions do *not* change place. It is true that with the eyes open they may *seem* to move, but this is only an illusion, the result of the contrary motion of all objects in the field of view. Real objects move because their images change their places on the retina while we look in the same direction, but the retinal brands which cause after-images cannot change their places on the retina. But now shut the eyes, so that there are no objects to plague us; then we find that after-images do not move by displacement of the axis of the eyes. There is only one case (that of the previous head) in which after-images follow the motions of the eye, although it is the commonest case. It is that in which the two eyes move together in the same direction. In other words, they follow the direction of looking, not the direction of the individual eye. But in displacement of the eye-axis by pressure we do not change the direction of the *looking* of the observer.

A convenient method of proving the above is as follows: Darken the experimental room slightly by closing the windows, but leave a crack between the shutters, showing a strip of bright sky. Now gaze with *one* eye, say the right, on the crack until its image is branded on the retina. If we now turn about and look at the wall in various directions the after-image, of course, follows all the motions of the eye. Even if we shut the eye and look about the field of darkness the after-image follows all the motions of the eye. But if, with eyes still shut and looking straight in front, without changing the direction of looking, we press in the external corner of the branded eye the after-image does not move. It still remains directly in front.

We have given this experiment as most convenient, but we may use a retinal brand produced by the setting sun with still more conspicuous results.

One more experiment to show the behavior of after-images in the movements of the eye. Gaze with *both* eyes on the crack of the previous experiment, until its image is strongly banded on the vertical meridian of both retinæ. On turning about and looking at the wall the after-image is distinctly seen and follows with exactness all the motions of the eyes in looking about. But now *converge* the eyes until they look at the root of the nose. Of course, each eye changes its direction at least forty-five degrees, but the direction of the after-image does not change. It is still directly in front. The reason is that, while each eye *individually* changes its direction, the binocular observer looks in the same direction, though at a nearer point. The two external images of the retinal brands cannot separate, as the images of an object do, because the brands are on corresponding points and have the same spatial representative and, therefore, must be seen single. This is the reason, as I have fully explained in my volume 'Sight,' pp. 199 and 200, why after-images cannot be used to test the motions of the eyes by rotation on the optic axis in *convergence*, although they are such accurate tests in *parallel motion*.

Joseph Le Conte.

Berkeley, Cal., July 29, 1897.

SCIENTIFIC LITERATURE.

Leitfaden der Praktischen Physik, mit einem Anhange, Das absolute Maas-system. Von F. Kohlrausch. Achte Auflage, B. G. Teubner, Leipzig.

Each new edition of this book has been characterized by such considerable additions that the modest guide to elementary laboratory work in physics, which first appeared under this title more than twenty-five years ago, has become a reference volume of some five hundred pages, as valuable to the advanced worker as to the beginner.

A laboratory manual should cover the entire field without undue specialization in any one direction, and without any omissions. Kohlrausch's book does this for physics more satisfactorily than any other. In fact, the manuals edited in America are too evidently, and often avowedly, nothing more than a compilation from the course given at the particular institution at which the author is teaching, and hence possess all the peculiarities and limitations of the work at that laboratory, and too often are of slight value elsewhere, with different conditions and facilities.

Kohlrausch sets forth not what is done at Würzburg or Strasburg, or even at Berlin, but what ought to be done under ideal conditions in a complete laboratory. The book is a guide to the instructor as to what experiments should be performed, as well as to the student as to how they are to be performed.

As regards the explanation of the operations, the author seems to have followed most successfully the principle laid down in the preface to this edition, "to carry the scheme, the explanation, and the setting-up of the apparatus for an experiment no further than is rendered necessary for the successful operation of a laboratory attended by a large number of students." He has achieved the happy mean between allowing the pupil to flounder too long in the working out of an experiment, and giving him such minute instructions that the necessity for originality of thought on his part is entirely eliminated.

It is difficult to select parts of this book as worthy of special mention, and yet certain subjects are treated in a manner in especially

agreeable contrast to that found in similar manuals. The calculation of corrections and the discussion of the effect of the errors of observation upon the result, as well as the methods of least squares and of approximation, will be found particularly clear, complete and useful. Indeed, much of the unique value of the book lies in its many suggestions as to simple manipulative methods, receipts and general good advice. It is these little 'tricks of the trade' that distinguish the successful experimental investigator from the helpless theorizer.

The body of the book contains concise, clear instructions for the determination of about every quantity that may be of interest to the experimental physicist, together with numerous demonstrations and illustrative examples. An excellent feature of this part is the frequent reference to the original investigations from which the methods were derived.

The absolute system of units is happily not the novelty to-day that it was when Kohlrausch introduced it into the Leitfaden; nevertheless his clear and complete exposition of its principles, its units and their values is very acceptable even now, and gives in some twenty pages the essentials of the whole matter.

The thirty pages of carefully selected tables form a very agreeable and striking contrast to some similar books recently offered in this country.

Some teachers have lamented the absence of pictures of instruments, but it would appear inconsistent with the general nature of the book if specific pieces of apparatus were represented. It is better and probably easier for a pupil to specialize from a diagram to any corresponding instrument than from one instrument to another of radically different appearance, though similar in principle.

The English translation of the last edition of Kohlrausch is very good, but not so good as the original because the translator has permitted himself to make changes.

A Systematic Treatise on Electrical Measurements. By HERSCHEL C. PARKER. Spon & Chamberlain, London and New York.

Mr. Parker, confining himself to a smaller field, and rather to the technical than the theoretical aspect of the subject, has succeeded in following the good example of Kohlrausch and sets forth what such a course should teach, rather than what is taught under the conditions at Columbia.

Particularly valuable is the strictly systematic way in which the subject is treated, enabling the student to see at a glance the relative merits of different instruments and methods for the various measurements of the same class, and to choose the apparatus and method best adapted to his particular determination. Although giving the principle and construction of the latest forms of electrical measuring instruments, the author has done better than some others, who could not resist the temptation to use old cuts or introduce trade pictures of apparatus. His diagrams and outline cuts show the fundamental parts most satisfactorily and enable the reader easily to imagine the brass and hard-rubber accompaniments. The book is sure to be useful to the electrical engineer, as well as to the investigator, who will gladly welcome future editions with the revision and additions which the author seems to feel desirable.

WILLIAM HALLOCK.

Bibliography—A Study of Resources. CHARLES SEDGWICK MINOT. In *Biological Lectures delivered at the Marine Biological Laboratory of Wood's Holl in the summer session of 1895.* Boston, Ginn & Company. 1896. P. 149–168.

Short surveys of the present standpoint of bibliography from the point of view of the special sciences should prove advantageous both for the specialist and for the bibliographer. To the former such surveys would be mainly useful in giving him a systematic guide through the mass of publications which he must work over to find the particular literature needed for his investigations. To the latter—I am here thinking especially of the librarian—they would make plain the connection of bibliography with the special sciences of which bibliography is the handmaid. Professor Minot's account of biological bibliographical literature is a good example of what such a survey should be. He does not drown his subject in enumeration of details, but describes and considers the main

guides to biological literature. One could wish to have as an appendix to the lecture a list, with full bibliographical details, of the publications treated, showing especially the connection and succession of the numerous German *Jahresberichte* and *Anzeiger*. As it is, however, the lecture makes not only an admirable introduction to biological bibliography, but also very interesting reading. The bulk of the lecture is devoted to the bibliographical publications in question, which are divided into four classes: (1) Standard bibliographies; (3) Incidental bibliographies; (3: a.) Current bibliographies appearing annually; (3: b.) Current bibliographies appearing at intervals of less than a year. As an introduction Professor Minot gives an interesting account of his own methods of dealing bibliographically with his literary material and with his library. In connection herewith he also gives some few rules for dealing with a scientific subject from the point of view of the bibliographer, emphasizing that the *title* of an article 'should be as brief as possible and nevertheless indicate the contents;' that *a table of contents* should be used in longer articles, say of 40 to 100 pages; that *reprints* should have the paging of the original publications; and that *references to other authorities* should be carefully arranged.

Aksel G. S. Josephson.

The John Crerar Library, Chicago.

Indiana—A Century of Changes in the Aspects of Nature. A. W. Butler. President's Address. From Proceedings Indiana Academy of Sciences, No. V., 1895.

This pamphlet of a dozen pages gives an interesting account of the changes in the natural conditions in Indiana, brought about by the advent of the white man, the passing away of the virgin forests, the destruction of the herds of buffaloes, the elk, the flocks of wild turkeys, the pigeons and many more.

In the valleys of the Wabash and Whitewater there were magnificent forests of deciduous trees, which probably could not be surpassed anywhere in America. Forty-two trees in the Wabash valley attained a height above one hundred feet, the tallest recorded being a tulip poplar (*Liriodendron tulipifera L.*), 190 feet high. The bison or buffaloes had well marked roadways in some of the river valleys, along which countless thousands passed annually, chiefly on their journey to and from the Big Bone Lick, in Kentucky. Elk and deer were common, bear and wolves quite abundant, beaver were found in many localities, while the wild cat Canada porcupine and panther were numerous. Wild turkeys and pigeons and the beautiful little Carolina paroquets were more than abundant, but have been almost, or in a great measure, exterminated. The hog assisted in the killing of the rattlesnakes and copperheads.

Thus, with the aid of the gun, of fire and the axe, was the land, all things being considered, speedily made ready for the plow, and a new life of sparrows, of little snakes, humble bees and grasshoppers took possession of the fields. More than half of humanity will declare that the destruction was unavoidable and even commendable, but we trace a spirit of regret running through all of Mr. Butler's admirable address, and this speaks for the rest of mankind, who would fain have saved a tract of virgin forest where they might resort to contemplate some of the wonders of the world.

W. T. Davis.

NEW BOOKS.

System der Bakterien. W. Migula. Jena, Gustav Fischer. 1897. Pp. viii + 368 and 6 plates.

Citizen Bird. Mabel Osgood Wright and Elliott Coues. New York and London, The Macmillan Company. 1897. Pp. xiv + 419. $1.50.

Some Unrecognized Laws of Nature. Ignatius Singer and Lewis H. Berens. New York, D. Appleton & Co. 1897. Pp. xvi + 511. $2.50.

Les huiles Minérales. François Miron. Paris, Gauthier-Villars et fils. Pp. 194.

Bulletin de la Société Belge de Geologie de Paléontologie et Hydrologie. Année. 1895. Vols. IX and X. Brussels, Polleunis et Centerick. 1895, '96, '97.

The University Geological Survey of Kansas. Erasmus Haworth. Topeka. 1897. Vol. II. Pp. 318.